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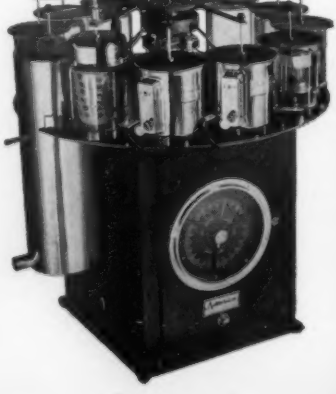

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The Work of Many People

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A MODERN technical and scientific development is rightly considered a wonderfully complex and difficult undertaking. The final intricate product has evidently required the greatest refinements of the art of engineering. The engineering phase has to be preceded by an experimental period of trials and adjustments, and even the very conception and theory of the device are rooted in many scientific thoughts and a mass of detailed calculations. Hundreds of ideas and thousands of technical skills are required for success. The hydrogen bomb is an achievement of this kind. It is the work of many excellent people who had to give their best abilities for years and who were all essential for the final outcome.

The story that is often presented to the public is quite different. One hears of a brilliant idea and only too often the name of a single individual is mentioned. This picture is both untrue and unjust. If one emphasizes the interaction of many different minds, one comes closer to the real life and the real excitement of exploration.

Over a number of years I have been closely associated with the development of the hydrogen bomb. I would like to attempt to give a picture of the many-sided efforts that went into this work. I cannot do so with any completeness. I can write only about those aspects of which I happen to be best informed: the conception, the theory, and the calculations. In the nature of things these represent only the beginning of the actual development, and they are not by any means the most important part of the work. I hope that there will be an occasion for others to tell the part of the story where tangible structures started to take the place of fantasies, sketches, and the long rows of formulas and figures.

The story cannot be rightly told without mentioning many of the people whose contributions made the hydrogen bomb possible, but it is even more difficult to attempt any kind of evaluation of the importance of each contribution. I shall mention names and incidents merely as examples of the kind of work that is needed in the close cooperation of which scientific and technologic developments consist. Perhaps this story will recall to some the adventure of trying to do what at one time seemed impossible.

The parts of the story that are most worth remembering are the positive contributions rather than the many mistakes that always necessarily occur in a complex undertaking. However, nature is patient and in the end only those mistakes count which in turn helped to point a way toward the correct ideas. It is the scientific tradition to emphasize what was good in a development, and it is this kind of tradition that makes the history of science so inspiring and accounts for much of the good fellowship among scientific workers.

The Sun and Other Stars

George Gamow escaped from Soviet Russia in 1933 and came to George Washington University in 1934. He had many interesting stories to tell. One of them is the following.

Six years before his arrival in the United States, he reported in the U.S.S.R. Academy of Sciences a paper by the British physicist, Atkinson, and the German physicist, Houtermans. They suggested that the apparently inexhaustible store of energy radiated by the sun and by other stars is due to reactions between atomic nuclei. These particles, tiny even compared with atoms, are known to contain an energy a million times greater than that released in chemical reactions or explosions. Yet they are able to release this energy only when they come in contact with each other. Their electric charges usually prevent contact between them and thus the energy that they have been carrying for billions of years is preserved. In the deep interior of the stars, however, exceedingly high temperatures exist. Owing to the thermal agitation, occasional collisions between the nuclei do occur, and these nuclear reactions ultimately give rise to the brilliance of the stars and to the radiation of our sun.

After Gamow finished his lecture he was approached by a very high Soviet official, Bukharin. By that time Bukharin had lost his real influence and had the job of keeping an eye on scientific developments. A few years later he was to be executed. After the lecture he asked Gamow whether nuclear processes similar to those occurring in the sun could be harnessed to some direct application here on earth. He offered to turn over to Gamow the Electric Works of Leningrad for a few hours at nighttime if that would help in the job. Gamow said that the practical job could not be done, but he remembered this occurrence and he kept his interest in the question of stellar energies.

Of course, we possess no direct knowledge about the interior of stars. Yet astrophysicists, starting with Eddington, had a pretty accurate knowledge of the conditions in those completely inaccessible regions. It may be puzzling to hear that science, which is practically unable to predict properties of matter in its common form encountered on earth, should be able to state with high accuracy how matter behaves inside the stars. The reason is a simple one. At the relatively low temperatures prevailing around us, the properties

of materials are determined by a sensitive balance between the attractions of the constituents of atoms and the energy of motion of these same particles. In the stellar interiors the temperatures are extremely high and the balance is destroyed. The atomic constituents —that is, the electrons and the nuclei—rush around at high velocities along straight lines, and the forces between these particles have little effect upon their motion. Thus matter, which in our common experience has many intricate and varied appearances and properties, behaves in a uniform and predictable manner in the inside of the hot gas balls which we call the stars.

The stars lose energy continuously and this energy must be replenished. Atkinson and Houtermans merely pointed out that the most probable source of this energy is the atomic nucleus itself. It was as yet unclear which of the great many possible reactions between nuclei give rise to the solar and stellar energy.

Gamow, with his wonderful sense for interesting problems, and with his contagious curiosity about the structure of the universe, stirred up quite a few of the physicists who had previously considered the interior of stars a too remote question. This soon led to the exploration of thermonuclear reactions, a long word which now has become quite familiar and which means the reaction of nuclei occurring at high temperatures. At that time, the late 1930's, the discussions and work were carried on with no idea of any practical applications. It was done merely to satisfy what to many would appear idle curiosity.

The first candidate for any thermonuclear reaction was the lightest of elements, hydrogen. In the sun and the stars this element appears to be by far the most abundant. Nuclei of hydrogen, by fusing into bigger nuclei, could release energies that are rather big even when compared with energies of most nuclear reactions. What is most important, hydrogen nuclei carry less charge than any other nucleus and therefore can approach each other more easily. Under the influence of Gamow's prodding, a small group of physicists and astronomers met at George Washington University and the Carnegie Institution in Washington in the spring of 1938. We had one of those disorganized discussions that we call a conference, which seem to lead nowhere but which often in the past had a great influence upon the development of science.

The conference did little more than pose the problems with some clarity, but the solution followed within the next few months. Hans Bethe, Charles Critchfield, and Gamow succeeded, not only in determining what reactions keep the stars going, but also in reconstructing how stars develop, change their appearance, and finally exhaust their sources of energy. The most remarkable part of this job was done by Bethe, who made a systematic study of all conceivable thermonuclear reactions, cataloging all the relatively meager experimental data of that day and supplementing them by wonderfully enlightened guesses about all the relevant nuclear reactions not yet experimentally explored. He found that, in addition to the possibility of hydrogen nuclei reacting with each other, one has to consider the reactions between hydrogen and carbon nuclei. His treatment of these reactions was so complete that in the next decade nothing useful could be added to his enumeration. Gamow had invented a new kind of game for the physicists, and Bethe proved to be the champion at it.

Conception of the Super

No one expected to be able to approach the conditions of the solar interior in any of our experiments. No container would have withstood the temperatures; no familiar source could deliver the energy in the necessary concentrated form. Then, in December 1938, Otto Hahn and Lise Meitner discovered fission.

It is now well known how fission releases energy. The neutron, a nuclear particle that carries no charge, can approach any nucleus even when no high temperatures are present. Some of the heaviest nuclei split under the impact of a neutron and produce more neutrons in the process. Thus a chain of reactions results, and the immense temperature and pressure of an atomic bomb can be produced.

Several years before Hiroshima, scientists started to wonder whether the high temperatures that were expected to occur in an atomic bomb could be utilized to start reactions similar to those that are proceeding within the sun. To me, this idea was first mentioned with some emphasis by Enrico Fermi. He proposed, in particular, to consider the reactions of heavy hydrogen rather than the reaction of the normal abundant light hydrogen. This heavy hydrogen, or deuterium, is present in ordinary hydrogen in the amount of 1 part in 5000, but it can be separated from the light hydrogen by some processes that are not altogether too costly. Furthermore it was known that the heavy hydrogen nuclei react with each other much more easily than those of light hydrogen. Therefore, the substitution of deuterium for hydrogen would be a long step toward realizing thermonuclear reactions under experimental conditions.

At that time, in the spring of 1942, both Fermi and I were at Columbia University. Physics had moved closer to the grim realities of war. Many of us had started to work on the fission bombs. It had become clear that these atomic bombs would be powerful but expensive. If deuterium could be ignited, it would give a much less expensive fuel.

After a few weeks of hard thought, I decided that deuterium could not be ignited by atomic bombs. I reported my results to Fermi and proceeded to forget about it.

In the early summer I found myself at the Metallurgical Laboratory of Chicago and in the company of Emil Konopinski, another physicist who had started to work on atomic energy. In the bustling laboratory of Chicago we were newcomers and at least for a few days we had no concrete job. I suggested that we go over my arguments about the thermonuclear reactions and that we make a conclusive

written report that heavy hydrogen would be of no use in bombs. The more we tried, the harder it seemed few days we found some loopholes that seemed to into arrive at a definite conclusion. In fact, within a dicate that deuterium could be ignited, after all.

In the meantime, Robert Oppenheimer gathered around himself in Berkeley a small group of theoretical physicists for the purpose of investigating the properties and behavior of atomic bombs. This group included Van Vleck, Felix Bloch, Stanley Frankel, Bethe, and Robert Serber. Konopinski and I joined the group when it was just being formed, and all of us were soon engaged in the distant but absorbing question of whether deuterium could be exploded.

It is hard to describe the intensity and the fascination of the discussion that followed. We were again dealing with conditions of high temperature completely unknown to experiment but open to theoretical predictions because of the very simplicity of the types of motion occurring under those conditions. The experience proved perhaps even more challenging than the previous discussion about the interior of the sun. Here we were not bound by the known conditions in a given star but we were free within considerable limits to choose our own conditions. We were embarking on astrophysical engineering.

As fact after fact emerged and was clarified, the prospects changed. One day the job looked hopeless, the next day it seemed easy, only to turn out again to be practically impossible on account of some considerations that had not been previously included. All of us contributed to the discussion, but without Konopinski and Bethe no real progress would have been made. I remember particularly the suggestion of Konopinski that the reactions of tritium should be investigated. At that time it was a mere guess. It turned out to be an inspired one. Bethe subjected all the relevant factors to the same kind of exhaustive scrutiny by which he had clarified the thermonuclear reactions occurring in the stars. By the middle of the summer of 1942, we were all convinced that the job could be done and that it would be relatively easy.

The spirit of spontaneity, adventure, and surprise of those weeks in Berkeley was never recaptured for me in the many years of hard work in which atomic bombs were developed. As the problems inevitably grew, as they came closer to the realities of engineering and hardware, exploration had to be replaced by schedule and spontaneous exchange of ideas by organization. I am sure that all the participants in those discussions still remember vividly the days when we thought that the atomic bomb could be easily used for a stepping-stone toward a thermonuclear explosion, which we called a "Super" bomb.

Certainly Difficult, Perhaps Possible

When Los Alamos was established in the spring of 1943, the exploration of the Super was among its objectives. Within a year, however, the picture changed completely. This was due to the fact that both the atomic bomb and the Super bomb proved to be more difficult than had been expected. Our discussion of the thermonuclear reactions proved to be incomplete, and it became clear that to make a Super would be difficult, if not impossible. At the same time, it became clear that the construction of the A-bomb was a much bigger job than we anticipated, and yet this had to be done before our enemies could do it. That it was done in time to have an influence upon the war was to a great extent due to the leadership of our director, Oppenheimer. He knew what was going on in every corner of the big laboratory and was prompt both in his understanding and in his encouragement. In his office there was a poster with Lincoln's picture, carrying the modified quotation, "This world cannot exist half slave and half free." It was hardly necessary, and yet helpful to remind everybody in the laboratory of this fact. We had to win the war and there was no time for the Super.

In spite of the urgency of the situation, Oppenheimer did not lose sight of the more distant possibilities. He continued to urge me with detailed and helpful advice to keep exploring what lay beyond the immediate aims of the laboratory. This was not easy advice to give, nor was it easy to take. It is easier to participate in the work of the scientific community, particularly when a goal of the highest interest and urgency has been clearly defined. Every one of us considered the present war and the completion of the A-bomb as the problems to which we wanted to contribute most. Nevertheless, Oppenheimer, Fermi, and many of the most prominent men in the laboratory continued to say that the job at Los Alamos would not be complete if we should remain in doubt whether or not a thermonuclear bomb was feasible. Furthermore, the purely scientific aspects of the Super were so fascinating that the problem continued to attract attention even in the hectic days in which our efforts on the atomic bomb approached completion and success. Thus in early 1945 a small but very able group started to concentrate its efforts on the thermonuclear bombs.

Most of this work continued to be pure theory, but there was less discovery and more quantitative evaluation. A number of talented young people joined our group. One of Bethe's students, Henry Hurwitz, proved that he had learned from his professor how to be systematic and ingenious. Two students from George Washington University, Geoffrey Chew and Harold Argo, interrupted their studies and came to Los Alamos to help us. Anthony Turkevich from Chicago contributed his knowledge of the theory of chemical reactions. Rolf Landshoff, a refugee from Germany, was the only one of the group who was going to stay at Los Alamos uninterruptedly from those days up to the present time. Two mathematicians, Stan Ulam and Jack Calkin, started to make calculations which even to a theorist seemed abstract. Nicholas Metropolis became interested in the use of computing machines, which in the later development turned out to be of great importance.

The experimental approach was not completely

neglected. Early measurements by John Manley, Elizabeth Graves, Marshall Holloway, and Charles Baker were continued by Egon Bretscher and other members of the British contingent. They, as well as our other British friends, participated without any restrictions in our great common work at Los Alamos.

Some of the most famous men in the laboratory kept in very close touch with our work and helped with frequent suggestions and criticism. One was Fermi, a physicist equally eminent in theory and experiment, the other was John von Neumann, one of the rare mathematicians who can descend to the level of a physicist.

In spite of all these contributions, no definite answer was reached. As the months went by, we still did not know whether the job could be done. But, paradoxical as this may seem, our very lack of certainty was based on a broader and more secure foundation. At the same time, all these people became acquainted with the increasingly complex arguments, and thus many of them could prepare themselves for further contributions in the future.

The most important part of all this work, however, was focused on one man, Konopinski. It was he who brought newcomers up to date, who made sure that none of the questions of which we were aware should go unexplored, and who finally made sure that our accumulating knowledge was preserved in clear and usable documents. Together with a young physicist, Cloyd Marvin, Jr., he also completed a strange and important task. He proved that a thermonuclear reaction, even if initiated on the earth, could not spread under any circumstances. It was necessary to prove, and he did prove, that the Super bomb could not ignite the atmosphere or the ocean. Later, his work was reviewed by one of the most conscientious, meticulous and painstaking physicists, Gregory Breit. It was clearly necessary to prove this point beyond the shadow of any doubt and it was so proved.

Then, in the summer of 1945, the work of the laboratory culminated in complete and terrifying success. The war was ended and the temper of the country and of the physicists seemed to preclude any further great efforts on the thermonuclear bomb. Some members of the wartime group, however, stayed on to prepare a summary review of the possibilities. It was Frankel and Metropolis who worked hardest and longest in preparing this report on the feasibility of the Super. The verdict was: Difficult, but with hard work, hopeful.

Hibernation

For several months after the end of the war it seemed likely that the Los Alamos Laboratory would be discontinued. Such an event would have been most dangerous for the security of the United States. That it did not happen was due to the effort of a few determined people who considered it their duty to try to keep Los Alamos alive, whatever the odds might be. The man whose leadership was crucial in those days was the new director, Norris Bradbury.

To keep Los Alamos alive was an uphill fight which remains clear in the memory of both those who stayed and those who left. One nontechnical event of great importance which all of us remember was the water shortage. In the fall of 1945 the snowfall came late, but frost came early. The water pipes froze and soon water had to be brought up the hill in trucks. We lacked ample water, one of the vital elements of civilization; this great discomfort continued into Christmas of 1945 and beyond. Los Alamos was a town of young people and there were many babies. Mothers started to wonder about dangers of epidemics, which fortunately never materialized. Many who had hesitated decided to leave Los Alamos. I recall one detail that may seem insignificant. During the war I developed, somewhat to my own surprise, two affections: a liking for strangers and the love of green grass. Both were in short supply. In the water shortage of 1945 the grass was dying.

Throughout all these difficulties Bradbury stayed on, kept smiling and encouraged others to stay with the job. Without his dedicated work the Russians would now be ahead of us in the atomic race.

I was one of the majority who returned at that time to academic work. The very success of the atomic bomb had raised some obstacles to the continuation of work on the thermonuclear weapon. There were those who felt that it would take a lifetime before the brilliant achievements of the war work could be improved. Of our thermonuclear group, only Landshoff remained, and most of his work was required for more immediate problems. Ulam left the laboratory for a short period and then returned to develop the Monte Carlo method, a highly successful procedure to deal with statistical problems by investigating individual happenings rather than the mass of the data. This fine work, however, was unrelated to the work on the Super. Ulam's contributions in that field came later. Thus, of the small group of experts whose skill was developed during the war, not one continued to devote his full time and energy to the next big problem in atomic weapons.

However, the idea of the Super bomb survived as a challenge and as a future task for Los Alamos. An exceedingly small group, headed by Robert Richtmyer, started to take over where others left off. They kept the spark alive, and their work should therefore be particularly remembered. In the following years I made many visits to Los Alamos and kept in close touch with the work of these people. From the very beginning this work had assumed a new direction and acquired a new style.

In the development of the atomic bomb, use of automatic computing machines had played an important role. It was essential that atomic bombs should be available without a lengthy preliminary period of experimentation. Furthermore, small-scale experimentation, similar in function to that of a pilot plant in industry, was out of the question in connection with atomic bombs: If you try to make a small atomic explosion you are likely to get no atomic explosion. Therefore, theoretical predictions had to be particu-

larly well considered and carried out in meticulous detail. This would have been most difficult without the extensive use of big computing devices. In the postwar period the theorists of the Super bomb turned their interest toward the most advanced computing machines.

In the months following the explosion of the first atomic weapons, Frankel and Metropolis started work on the new fast electronic computer in Philadelphia, the ENIAC. Their work was soon taken over by Richtmyer, Foster and Cerda Evans, and a few others. At the same time von Neumann, together with many excellent people throughout the country, was working hard at further plans and improvements of the fast electronic computers. After an absence of more than a year, Metropolis rejoined Los Alamos and started to build the MANIAC (which is supposed to mean Mathematical Analyzer Numerical Integrator And Computer). Richtmyer became interested in these computational methods and became one of the real masters in handling the machines.

A fast computer, while very efficient, needs detailed instructions, and it is quite an art to transform a mathematical problem into a set of symbols that will make the machine operate properly. Furthermore, this set of symbols is hardly ever free from errors, so that after a problem is fed into the machine the first set of answers usually makes no sense. There must follow a period of "debugging" in which the very answers obtained lead to the discovery of mistakes in the original instructions. To make matters worse, the machine itself makes occasional mistakes and these may get confused with errors in the instructions.

In the normal course of operating a computing machine, several people cooperate: the physicist who sets up the problem, the mathematician who provides the rigorous formulation, the coder who "explains" the problem to the machine, the machine operator who straightens out the errors, and then the physicist again who uses the answers to set up the next problem. Richtmyer argued that all these operations can be performed by a single man more efficiently, and he proceeded to demonstrate that this could be done. This style of computation is practiced at present by many able people.

Work on a difficult subject such as a Super bomb depends on the mutual help and encouragement of at least a few people. When the group is small there is danger that the effort will cease altogether; but in the case of the thermonuclear effort the small group of people kept on increasing, even though the increase was slow. The evident importance and scientific interest of the problem caught and held the imagination of additional workers. Frank Hoyt, a professor at the University of Chicago, visited from time to time to help out. Later he joined Los Alamos on a permanent basis. His quiet and devoted work contributed much to the sustained effort. Some of my students at Chicago also became interested and joined the laboratory after completing their graduate work. Harris Mayer and John Reitz were to make lasting contributions, and Marshall Rosenbluth eventually became one of the key men who carried the calculations to their successful conclusion.

I particularly remember a short visit from Lothar Nordheim from Duke University. He is a man who is likely to sit through a dinner among friends without participating in the conversation and who is likely to come up toward the end with just one remark. Later it turns out that one can recall, of the whole evening, only Nordheim's comment. His work proved to be of the same quality. He started it in the lean years. Later he joined Los Alamos for an extended period, always working inconspicuously either alone or with very few people. In the beginning his effort did not seem to amount to much. In the end it turned out to be one of the really significant contributions.

In the middle of 1949 I went back to Los Alamos to spend a considerable length of time there. In the preceding 3 years the laboratory had recovered to a remarkable extent. We had lost somewhat in competence as compared with the high-powered crew that was available at the end of the war, but we had progressed in some ideas, and the technique of big-scale computations had been developed. Then the Soviet bomb brought the realization that the arms race was no longer a possibility but a frightening reality.

The Crisis

It is clear that the hydrogen bomb would not have been built except for the efforts of a considerable number of people whose contributions ranged from political decisions to organization and on into the scientific work. My knowledge and appreciation are, of course, greatest in connection with this last phase, which is closest to my own interests. I shall talk only about the work that concerned equations and atoms and will stop short of blueprints and bombs. About the latter I do not know enough to give a just description. About the political decisions that had to be made after the explosion of the Soviet bomb, I know even less. My direct experience is limited to the few occasions when I was asked to give my opinion on technical possibilities and probabilities; but I feel that great gratitude is due to the men who in those difficult weeks arrived at the correct conclusions.

The decision concerning how to respond to the threat of a Soviet bomb was not an easy one. At Los Alamos there was a widespread feeling that the laboratory should turn to the development of the hydrogen bomb. During the war it had been understood that this possibility must be explored. Many people felt that the time for this had come. In the administration of the laboratory the first to make a concrete and determined effort toward planning a big-scale approach was Darol Froman. A 6-day work week was adopted upon the urging of Holloway and others.

However, the center of the hydrogen bomb activity remained for some time in the theoretical group on which plans had to depend. This group, although small, was of high quality and expanded rapidly.

Much of the credit for its build-up and successful activity must go to its able organizer and leader, Carson Mark.

Frederic de Hoffmann had joined the laboratory in the early part of 1949. Even before the Soviet explosion he felt that the hydrogen bomb must be our main task. Now he acted like a man who has been freed from a terrible restriction. He was the kind of associate who would never let me forget the importance of the job that we were doing, and I am sure that my own effectiveness depended greatly on his skill, devotion, and example. Nordheim joined the laboratory to explore further the consequence of his earlier work. John Wheeler from Princeton interrupted a well-deserved sabbatical leave in Europe and, together with some of his students, plunged into furious and effective activity. Roy Goranson helped to maintain contact between the theory and the practical execution. James Tuck shelved his greatest interest, peacetime applications of atomic energy, and devoted himself to the urgent phase of the program. Together with a group of able experimental physicists, he made some measurements of vital interest to the thermonuclear program. Von Neumann and Fermi helped, if less frequently, no less effectively than during the war.

Our most urgent task was to reconsider with the greatest possible rigor the favored design of the hydrogen bomb. We intended to do this with the help of the high-speed computing machines. The best of these, however, were not yet operating at the time, and the calculation was set up on the ENIAC, which in the intervening years had been moved to the Aberdeen Proving Grounds. Ulam, with the able help of another mathematician, Cornelius Everett, undertook to execute the same job by straightforward hand computation. The next few months saw an amazing competition between the tortoise and the (electronic) hare.

The big modern computing machines open up possibilities of complex calculations which seemed to be beyond our reach only a few years ago, but real mathematical ingenuity, coupled with hard work, can on some occasions overcome computational difficulties with even greater success than the best apparatus so far invented. This is precisely what happened in the case of Ulam's calculation. It proceeded with a speed that surpassed all expectations. Results were available even before the lengthy instructions to the machines had been completed. Those who like to contrast ingenuity and endurance of the human brain with the ligntning speed of standard operations on a machine will be able to conclude: In a real emergency the mathematician still wins—if he is really good.

Ulam's first partial results were disquieting; the more complete answers, most discouraging. I felt at the time that these calculations, which seemed to be in conflict with earlier results obtained on machines, were hard to believe. In actual fact they were correct, and they served a most important purpose in alerting us at an early date to the difficulties that we were facing. A few weeks later, when the more detailed and accurate results from the machine were in, it became completely clear that the plans which we had considered most hopeful had to be revised.

The probable success of a radically new device such as the hydrogen bomb is not likely to depend on one particular line of approach. Real progress depends on the complete understanding of the field and on the efficiency of methods that apply this understanding to detailed designs. It furthermore depends on experiments and tests to compare the theoretical knowledge with reality. The work of the years that had passed since 1942 had left us with a great store of knowledge of the principles and methods, but the calculations of Ulam and Everett deprived us of the best example of a device to which we could point and say: This is how we actually want to do it.

It is clear that there had to be discouragement. The remarkable thing is that the majority of the people engaged in the work at Los Alamos kept on working hard and with a good spirit. This included almost all the theorists who had been working on the project.

The plan for a complex apparatus like a hydrogen bomb is not tied to one single design. There are many possibilities and each possibility can be handled in many different ways. In early 1950 we had 8 years of fantasies, theories, and calculations behind us. We also had some significant measurements performed in the laboratories on the basic process, but we had no experience whatsoever that would tell us whether or not our assumptions and general ideas had anything to do with the behavior of real objects. It had become most urgent to come back to solid ground by establishing a connection between theory and practice. In other words, we needed a significant test. Without such a test no one of us could have had the confidence to proceed further along speculations, inventions, and the difficult choice of the most promising possibility. This test was to play the role of a pilot plant in our development.

The first immediate job was, therefore, to make detailed calculations concerning the test. Because of the shortage of high-speed computing equipment, much of this arduous work still had to be carried out by hand. Under the supervision of Wheeler, Landshoff, Richtmyer, and some new recruits among whom were Conrad Longmire, Rosenbluth, and Burton Freeman, an incredible amount of numerical data was turned out by the untiring work of the people in the Los Alamos computing division. Thus, the comparison between the results of the test and the theory of thermonuclear burning could be anticipated with some measure of confidence.

In the second half of 1950 and in early 1951, the most complex kind of apparatus was being built in order to observe the results of the test. It is impossible for me to describe the excellent effort that went into this work. The device we were building was going to function for a minute fraction of a second. The observing equipment was going to be destroyed by the test explosion, yet delicate effects had to be recorded before the test apparatus was vaporized. We had to

find out not only what actually happened in this test but also which were the best observational tools to be used in future tests.

Under the direction of Alvin Graves, Frederick Reines, Jack Clark, William Ogle, and others, an intricate laboratory was built on Eniwetok. The Los Alamos effort was augmented by excellent crews from the Naval Research Laboratory and from the Radiation Laboratory of the University of California. Never before have so many experimental scientists believed in such a mass of complex calculations not as yet compared with any process in the real world.

During most of these preparations plans for an eventually successful device had to take a low priority. The most important thing was the test, which required such a great effort and which was to confirm or disprove our ideas. We had to establish beyond a doubt that thermonuclear burning was possible. The question whether it could be used in an economically designed weapon had to wait for many months. Yet, it would be a mistake to believe that in this period plans were not maturing. The most fruitful suggestions often occur when one is occupied with a different and urgent project, and many of the hard-working physicists contributed in offhand discussions their ideas, sometimes fantastic, sometimes practical, to what might in the end become a usable device.

However, the immediate thing ahead of us in the spring of 1951 was the test in the Pacific: Greenhouse. I do not know how many scientific experiments have been made under conditions as exotic or in a place as beautiful as was the setting for the first thermonuclear experiment. There must have been other events as strange, exciting, and unforgettable. What remains most clear in my mind is the contrast between the spectacular explosion, which in itself meant nothing, and the small piece of paper handed to me by my good friend, Louis Rosen, which showed that the experiment was a success. The test gave us the assurance that we needed. Our detailed calculations agreed remarkably well with the results of the test.

Success

All of us knew that after Greenhouse we faced the real decision: can a usable device be constructed or not? All of us worried about this question. Some made very specific plans. Wheeler set up a group in Princeton which was preparing for the long hard pull in the calculations that were to decide the issue. But the essential parts of the decision started to come faster than had been expected by anybody.

A few months before the Greenhouse test all calculations had to be completed, and at that time it became possible for many of us to devote our full attention to the problems of the construction of an actual bomb. This time the challenge found our group in Los Alamos fully prepared. Calculational techniques were developed to a high pitch. Half-examined ideas were lying around by the score. They had to be shoved aside for the sake of more immediate calculations. Now we had the opportunity to look at them in detail. A year had passed since the decision to go ahead at the fastest possible rate with the hydrogen program, and everyone was eager and anxious to come to grips with the real problem. Two signs of hope came within a few weeks: one sign was an imaginative suggestion by Ulam; the other sign was a fine calculation by de Hoffmann.

I cannot refrain from mentioning one particularly human detail in de Hoffmann's work. Since I had made the suggestion that led to his calculation, I expected that we would jointly sign the report containing the results. Freddie, however, had other plans. He signed the report with my name only and argued that the suggestion counted for everything and the execution for nothing. I still feel ashamed that I consented.

Even before the Greenhouse test it became evident to a small group of people in Los Alamos that a thermonuclear bomb might be constructed in a comparatively easy manner. To many who were not closely connected with our work this has appeared as a particularly unexpected and ingenious development. In actual fact this too was the result of hard work and hard thought by many people. The thoughts were incomplete, but all the fruitful elements were present, and it was clearly a question of only a short period until the ideas and suggestions were to crystallize into something concrete and provable. Both Los Alamos and the newly formed group in Princeton immediately started calculations on this new approach.

The calculations on the new plans, though still crude, were presented at a meeting in Princeton to the Atomic Energy Commission and its advisers shortly after the Greenhouse test. Even while this meeting was in session, fresh results from Wheeler's group were being brought in. This group, which was organized in a period of uncertainty, was gaining remarkable momentum and hope and their mood was contagious. In the Princeton meeting everyone clearly recognized that with a little luck, only a great deal of hard work stood between us and final success.

Now at last the high-speed computing machines started to play the significant role that had been foreseen a few years earlier. A somewhat modest but very efficient machine, the SEAC, was in operation at the National Bureau of Standards, and the director, Edward Condon, invited us to make use of it. With the help of this facility, initial details of the plans were ironed out in a few weeks rather than in tedious months. Soon even faster machines, including particularly the Los Alamos MANIAC, helped to speed our work, so that the calculations on the design could be carried through more thoroughly and in shorter time than anyone could have expected. The art of machine calculations was now shared by many of the leading theorists in Los Alamos. In the hands of Rosenbluth, Longmire, Nordheim, Freeman, and many others, speculations hardened into complete specifications. Wheeler's group at Princeton developed similar expert knowledge in an amazingly short time. John Toll, Kenneth Ford, and others not only helped to make sure of the success of the immediate plans

but started to contribute toward designs that were to bear fruit only in the more distant future.

In the fall of 1951 I left Los Alamos. I felt sure that everything was going to be done to construct a thermonuclear bomb with the greatest possible care and precision. The theoretical division under Carson Mark had grown into a most able outfit, which was to be joined for the critical months by Bethe. All of us felt that his presence would make sure that nothing would be forgotten in the preparations. Yet people kept worrying about possible difficulties, even dreaming about them, up to the time of the final test explosion. Thus, with Los Alamos furnishing the solid foundation and Princeton much of the drive and optimism, one could look with confidence into the future.

It was a great disappointment to me that I could not participate in the final phases of this magnificent undertaking. The main reason that persuaded me to leave Los Alamos was a conviction that this was an opportune time in which to start plans for a second weapons laboratory. Science, as well as any kind of technical work, thrives on friendly competition, on the fostering of different points of view, and on the exchange of ideas developed in various surroundings. It is only too easy for a single group to become fascinated by some special aspects of a development and to neglect other hopeful approaches. I felt that the safety of our country could not be entrusted to a single laboratory, even though that laboratory were as excellent as Los Alamos.

In the course of time this second laboratory was established at the Livermore site of the Radiation Laboratory of the University of California under the directorship of Herbert York. Its work so far has been mostly that of learning the difficult art of inventing and making nuclear weapons. All the magnificent achievements that have become in the meantime known to the world have been accomplished by Los Alamos. But in the intervening years a group of young experts has grown up in Livermore. The more they see that Los Alamos is a long distance ahead of them, the more eager they are to catch up. Having had the privilege of being associated with this young and vigorous group, I feel sure that the work at Los Alamos and Livermore will be mutually helpful to the two laboratories and will be of the greatest importance to the country as a whole. It is of no interest which of the two laboratories will be able to accomplish the most in the future. The only important thing is that each of them should do what it can and that together they should do what is enough.

The difficulties of the task of a weapons laboratory could be no more clearly illustrated than by describing the work of the last year that preceded the explosion of the first successful hydrogen bomb in Eniwetok. My knowledge of the details of this undertaking is not good enough, however, to justify any description or evaluation of the great work of this year, but I would like to mention the kind of difficulties that had to be faced.

Traditional engineering is thoroughly empirical. The usual sound practice is to make progress in small steps. A big plant is preceded by a small pilot plant. A full-scale device is not started until details have been checked on models. Work on atomic bombs makes it necessary to change this conservative practice. The final device is put together without any significant model experimentation. Tests, such as Greenhouse, can give guidance to the theorists, but they give little concrete support to the engineers. Dimensions, tolerances, strange materials go into the final design which would leave traditional engineers bewildered and helpless. Los Alamos had developed in the experience of many hard years an effective method of dealing with these grotesque difficulties of hardware. I can only guess how great these actual difficulties are, but I suspect that the greatest achievement in the production of the hydrogen bomb was not the conception or the invention but the execution. The man who was in charge of this undertaking was Marshall Holloway. I hope that at some time the story of this phase of the undertaking can be told, but the most important fact is this simple one: It was difficult and it was completely successful.

In October 1952 I was kindly invited to attend the explosion of the first full-scale device called "Mike." I would have liked to go, but it was clear that I would not have been of any concrete use in the Pacific. At the same time Livermore, only a few weeks old, was requiring the fullest attention of all of its members, so I chose the second best solution, which was much less expensive in money, effort, and time. I attended the first hydrogen shot by watching the sensitive seismograph in Berkeley.

In the morning of 1 November 1952 I was shown into the basement where the seismograph was operating. This seismograph is a recording instrument that writes with the help of a fine beam of light on a photographic film. The room was completely dark except for the tiny luminous spot that the pencil of light threw on the photographic paper. After my eyes became accustomed to the darkness, I noticed that the spot seemed quite unsteady. Clearly this was more than what could be due to the continuous trembling of the earth, to the "microseisms" that are caused by the pounding of the ocean waves on the shores of the continent. It was due to the movements of my own eyes, which in the darkness were not steadied by the surrounding picture of solid objects. Soon the luminous point gave me the feeling of being aboard a gently and irregularly moving vessel, so I braced a pencil on a piece of the apparatus and held it close to the luminous point. Now the point seemed steady, and I felt as if I had come back to solid ground again. This was about the time of the actual shot. Nothing happened or could have happened. About a quarter of an hour was required for the shock to travel, deep under the Pacific basin, to the California coast. I waited with little patience, the seismograph making at each minute a clearly visible vibration which served as a time signal. At last the time signal came that had to be followed by the shock from the explosion and

there it seemed to be: the luminous point appeared to dance wildly and irregularly. Was it only that the pencil which I held as a marker trembled in my hand? I waited for many more minutes to be sure that the record did not miss any of the shocks that might follow the first. Then finally the film was taken off and developed. By that time I had almost convinced myself that I must have been mistaken and that what I saw was the motion of my own hand rather than the signal from the first hydrogen bomb. Then the trace appeared on the photographic plate. It was clear and big and unmistakable. It had been made by the wave of compression that had traveled for thousands of miles and brought the positive assurance that "Mike" was a success.

What Next?

I believe that everyone who has worked on the hydrogen bomb was appalled by the success and by its possible consequences. I also believe that everyone who was closely or distantly connected with the effort and who made any contribution, great or small, had a clear feeling that the work was necessary in the interest of the safety of our country. To that extent I feel that all of us had an equal sense of satisfaction in the final success on 1 November 1952 at Eniwetok in the Marshall Islands.

In the whole development I want to claim credit in one respect only. I believed and continued to believe in the possibility and the necessity of developing the thermonuclear bomb. I feel that it was a great privilege that I could stay with it until a time at which the successful conclusion was in sight.

At the present time I find myself unhappily in a situation of being given certainly too much credit and perhaps too much blame for what has happened. Yet, I feel that the development of the hydrogen bomb should not divide those who in the past have argued about it but rather should unite all of us who in a close or distant way, by work or by criticism, have contributed toward its completion. Disunity of the scientists is one of the greatest dangers for our country.

The very size of our progress has opened up other dangers. We may be led to think that this accomplishment is something ultimate. I do not believe that this is so. Where the next steps will lead, I do not know. It is not likely that it will be just bigger bombs again. The world is full of surprises, and great developments rarely go along straight lines. But the skills and the knowledge that developed the A-bomb and the H-bomb can undoubtedly be turned to new directions, and we shall fail if we rest upon our accomplishments.

The greatest and most obvious danger of the hydrogen bomb is its destructive power. Some may think that it would have been better never to develop this instrument. I respect their opinion and I understand their feelings. There can be nothing more strong and definite than our desire for peace and I am sure that those who were most closely connected with the development of the new destructive weapons feel this at least as strongly as anyone else. But I also believe that we would be unfaithful to the tradition of Western civilization if we were to shy away from exploring the limits of human achievement. It is our specific duty as scientists to explore and to explain. Beyond that our responsibilities cannot be any greater than those of any other citizen of our democratic society.

It is clear and it is true that atomic bombs and hydrogen bombs are terrible and unprecedented, but so have been many other developments that past generations have faced. The construction of the thermonuclear weapon was a great challenge to the technical people of this country. To be in possession of this instrument is an even greater challenge to the free community in which we live. I am confident that, whatever the scientists are able to discover or invent, the people will be good enough and wise enough to control it for the ultimate benefit of everyone.

A Labile Precursor of Citrovorum Factor

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THE 4-amino antagonists of pteroylglutamic acid (PGA), such as Aminopterin and A-methopterin, apparently exert their effect by blocking the formation of derivatives of folic acid concerned as coenzymes with the transfer of single carbon units, and thus with the synthesis of several components of proteins and nucleic acids (*1*). Aminopterin was the first agent to show striking effectiveness in the treatment of acute leukemia of children (*2*). The availability of an organism, *Leuconostoc citrovorum*, ATCC 8081 (recently reclassified as a typical strain of *Pediococcus cerevisiae*, *3*), which requires a reduced derivative of PGA, has made possible the observations that PGA is reduced metabolically and that the formation of citrovorum factor (CF, N^5-formyl-5,6,7,8-tetrahydro-PGA) derived from PGA by liver preparations and by suspensions of bacterial or leukemic cells, is blocked effectively by Aminopterin (*1, 4*).

Although evidence was available that CF itself is

Table 1. Effect of heat and exposure to nitrogen and oxygen on a product derived from pteroylglutamic acid by cells of *S. faecalis*/A.

Exposure time for supernatant* (min)	Folic acid activity (*S. faecalis* 8043)		Citrovorum factor activity (*L. citrovorum* 8081)	
	Unheated (μg/ml of supernatant fluid)	Heated	Unheated (μg/ml of supernatant fluid)	Heated
0	54.0	55.0	4.10	19.4
Atmosphere of nitrogen at 37°C				
30	54.1	56.4	4.65	18.6
120	56.8	57.1	8.48	20.2
240	58.5	57.6	12.3	20.6
Atmosphere of oxygen at 37°C				
30	57.6	57.6	4.34	11.9
120	58.1	58.3	4.96	5.05
240	60.1	55.4	3.88	4.11

* Cells of *S. faecalis*/A [2.1 mg (dry wt.)/ml of incubation mixture] were suspended in phosphate buffer (0.2*M*, pH 6.5) containing glucose (0.02*M*), sodium ascorbate (0.01*M*), and sodium formate (0.01*M*). The cells were incubated anaerobically for 1 hr in plastic centrifuge cups, which were then capped and spun in a refrigerated centrifuge. Portions of the supernatant fluid were returned to the incubator under atmospheres of nitrogen and oxygen and were agitated constantly. Duplicate aliquots were removed at intervals and were frozen immediately. One of each pair of aliquots was heated at 120°C for 30 min. The samples were assayed for their activity for *S. faecalis* 8043 and *L. citrovorum* 8081 (*10*), using PGA and CF, respectively, as standards of comparison.

not a coenzyme (*5–8*), its close relationship to the functional cofactor(s) has been regarded favorably. In fact, recent investigations have suggested that the functional forms of the vitamin may be derivatives of tetrahydro-PGA carrying specific one-carbon substituents at the formate or formaldehyde levels of oxidation (*5–8*). Unfortunately, direct methods for the study of the effect of the antagonists on the formation of the coenzymes, as distinct from CF, have been lacking.

We have observed recently that following the incubation of either preparations of pigeon liver or suspensions of an A-methopterin-resistant strain of *S. faecalis* (*9, 10*) with PGA, the appearance of CF results from the nonenzymatic degradation of a more labile compound and that the formation of CF is inhibited by Aminopterin only indirectly. The lability of the product of the enzymatic reactions under aerobic conditions, and the conditions that favor its conversion to CF are features that require careful consideration in the interpretation of metabolic studies of the nature and activity of derivatives of PGA.

The product formed by the anaerobic incubation of PGA with cells of *S. faecalis*/A was exposed to atmospheres of nitrogen and oxygen with the results shown in Table 1. The supernatant fluid that was obtained after the removal of the cells in a refrigerated centrifuge did not have the ability to alter added PGA. Comparison of the heated and unheated samples of the supernatant fraction derived from the incubation of the cells with PGA indicated the presence, not only of CF, but also of a derivative of PGA that was converted to CF by heating in the manner described. Incubation in the presence of oxygen led to the disappearance of the heat-labile precursor of CF. However, in each case, the total activity of the solution for *S. faecalis* remained the same.

Under anaerobic conditions at 37°C the compound was converted gradually to CF, a compound that is stable upon exposure to heat or oxygen, under the conditions described. Since the formation of the labile compound by this anaerobic system required the presence of ascorbate, which stabilizes the oxidizable product, it is probable that the true rate of oxidation of the labile precursor of CF is greater than that indicated by this experiment. The presence of cysteine or glutathione did not duplicate the effect of ascorbate in liver preparations; however, in this bacterial system an effect of these compounds, similar to that of ascorbic, could be demonstrated.

The rate of formation of CF from its precursor (formed by the incubation of PGA with homogenates of pigeon liver) was more rapid at 120°C than at 100°C. Also, after the elimination of enzymatic activity by removal of the bacterial cells, the rate of formation of CF in the supernatant fluid derived from *S. faecalis*/A incubated with PGA was approximately doubled for each 10-deg increase in temperature from 40° to 80°C.

The evidence that CF can be derived from a precursor nonenzymatically implies that CF may very well not be a direct intermediate in the formation of the functional form of the vitamin derived from PGA. The occurrence of separate pathways for the formation of the cofactor from PGA and CF could account for the observation that an A-methopterin-resistant strain of *S. faecalis* required less PGA than CF for half-maximal growth (*10, 11*). We have found that Aminopterin exerts an effect that is observed as an inhibition of the formation of CF, but this is an *indirect* effect. Aminopterin effectively blocked the formation of the precursor but was without effect upon the subsequent formation of CF, per se. Blakley (*6*) observed that Aminopterin inhibited the serine-glycine interconversion in the presence of PGA and yeast extract, but not in the presence of tetrahydro-PGA.

Isolation of the CF formed by heating the filtrates of *S. faecalis*/A incubated with PGA yielded a material indistinguishable from synthetic CF (folinic acid-SF; leucovorin) with respect to absorption spectra, R_f values on paper chromatography, and stability in acid and alkali (*12*); like the CF isolated from liver (*13*), however, it was twice as active as the synthetic compound in promoting the growth of *S. faecalis* (*12*). Bioautograms using *S. faecalis* showed that in addition to PGA and CF, two folic acid-like compounds were present in the heated supernatant fluid. Fractions of the supernatant fluid obtained by the use of Dowex-1 columns contained N^{10}-formyl-PGA and

a compound that was degraded to N^{10}-formyl-PGA in neutral solutions (*12*).

The properties and chemistry of synthetic compounds related to CF have been described in detail (*14, 15*). The lability of the N^{10}-formyl derivative of tetrahydro-PGA may be contrasted sharply with the rather marked stability of the N^5-formyl derivative (CF). Thus, N^{10}-formyl-tetrahydro-PGA can be converted to CF by an anaerobic process involving heating. Such a conversion probably involves the intermediate formation of a compound having a one-carbon bridge linking the N^5- and N^{10}-positions. In neutral or mildly alkaline solutions such imidazoline compounds can yield CF under anaerobic conditions, or N^{10}-formyl-PGA under aerobic conditions. Upon exposure to oxygen, N^{10}-formyl-tetrahydro-PGA is readily oxidized to N^{10}-formyl-PGA, and the formyl substituent is readily removed by hydrolysis under mildly alkaline conditions. Recently Jukes (*16*) has directed attention to the instability of compounds in the folic acid series.

The labile compound formed from PGA in these studies would appear to be a derivative of PGA at the tetrahydro level of reduction, since it yields CF upon heating and is labile upon exposure to oxygen. We refrain from adding to the complex terminology related to folic acid-like compounds (*17*) by naming this labile derivative of PGA prior to a clarification of its chemical nature. For the sake of brevity, as well as utility, it is designated as CFX.

The results of investigations on the nature and activity of folic acid-like compounds occurring naturally in tissues or synthesized metabolically from *p*-aminobenzoic acid (PABA) or PGA are compatible with the tentative interrelationship between such derivatives shown in Fig. 1. We suggest that a distinction between the enzymatic and the nonenzymatic phases of the formation of these folic acid-like compounds is important to the interpretation of the experimental observations.

Certain microorganisms that require PABA synthesize from it compounds which appear to function as coenzymes and from which CF or PGA can be derived; the formation of such compounds by these organisms can be inhibited by sulfonamides. Although PGA and CF have been regarded as derived metabolically from PABA, there is substantial evidence to show that PABA is more efficient than either PGA or CF in promoting the growth of these organisms and in counteracting the inhibition of their growth by sulfonamides (*18–20*). Woods (*19*) proposed that PGA and CF are not direct intermediates between PABA and the coenzyme but that they could be converted to such intermediates with varying efficiency by some organisms and not at all by others. Since PGA and CF may represent relatively stable products formed by the degradation of a labile CF-like compound formed enzymatically from PABA, the activity or inactivity of PGA or CF would be related to the occurrence of enzymes capable of forming the labile cofactor-precursor from these stable derivatives. PGA and CF ordinarily do not counteract the chemotherapeutic activitity of sulfonamides, even though these compounds obviously are converted to functional cofactors in mammals, including man. This may be because PABA plays other roles of critical importance besides serving as a precursor of CF-like coenzymes. Consideration must also be given, how-

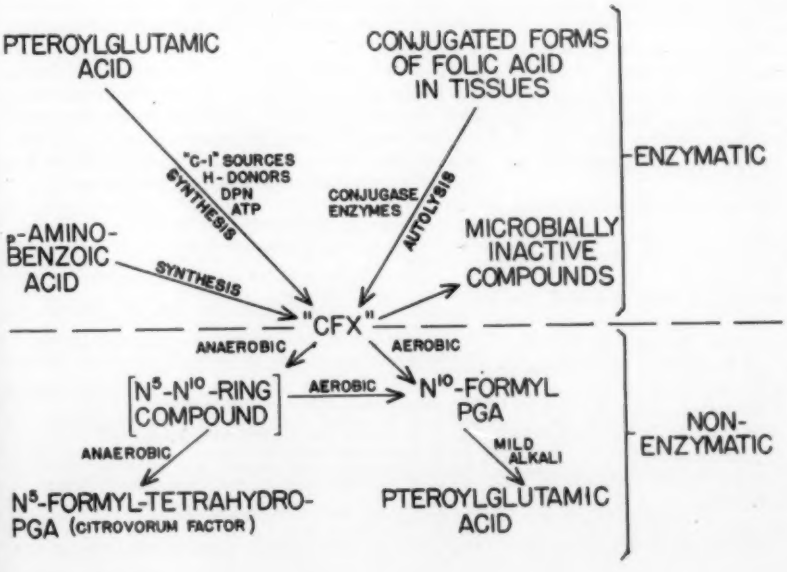

Fig. 1. Tentative interrelationships of folic acid-like compounds.

ever, to the possibility that parasitic organisms inhibited by sulfonamides may be unable to obtain benefit from the tissue coenzymes that are formed in animals from ingested folic acid-like materials.

Compounds having folic acid-like activity can be released by autolysis from forms occurring naturally in tissues, presumably by the action of the conjugase enzymes that degrade the polyglutamate compounds to forms that are available to the usual assay organisms, but the activity and role of these enzymes under physiological conditions is quite unknown. Keresztesy and Silverman (*13*) isolated CF from autolyzed horse liver and demonstrated its similarity to the active isomer of the synthetic form of CF (*21*). When liver homogenates were incubated under anaerobic conditions, these workers observed that after a brief autolysis the CF activity was a small proportion of the total folic acid-like activity, whereas upon continued autolysis the proportion of folic acid-like activity measurable as CF increased greatly (*22*). An attempt to separate the precursor of CF resulted in the isolation of a compound that was converted to PGA on alkaline hydrolysis and that had properties similar to those of N^{10}-formyl-PGA (*23*). It would appear that partial autolysis of liver resulted in the release of a substance that could yield folic acid-like compounds without activity for *L. citrovorum*, whereas upon continued incubation under anaerobic conditions the stable product formed was apparently CF.

In metabolic systems the apparent amount or activity of compounds in the folic acid series may involve the interconversions indicated in Fig. 1 and also the inactivation of these compounds. The enzymatic cleavage of PGA and CF has been reported (*24, 25*). In 1949 Welch *et al.* (*26*) suggested that xanthopterin, which, when incubated aerobically with homogenates of liver, apparently increases their folic acid-like activity (*27*), may function as an inhibitor of the enzymatic inactivation of precursor compounds. More recently, Silverman and Keresztesy (*23*) observed that the precursor of CF in autolyzing liver could be converted to a microbially inactive product by a DPN-dependent reaction and that this inactivation was blocked by sodium arsenite, Antabuse or xanthopterin.

The metabolic alteration of PGA to a functional form requires the availability of suitable donors of carbon, hydrogen, and, for reactions involving the utilization of formate, a source of energy. Greenberg (*7*) reported that a compound that activated the transfer of formate to an acceptor compound (4-amino-5-imidazole carboxamide ribotide) in the synthesis of inosinic acid, can be derived enzymatically from dihydro-PGA with the mediation of ATP, DPN, and formate. During an attempted isolation, however, a compound that was identified as N^{10}-formyl-PGA was obtained. Nichol (*28*) has observed that the formation of CF from PGA by "sonic" extracts of *S. faecalis*/A requires the presence of ATP and DPN in addition to formate or serine. Kisliuk and Sakami (*8*) have noted an important distinction between formaldehyde and formate as sources of carbon for the formation of the β-carbon of serine. In the presence of tetrahydro-PGA, ATP was required for the utilization of formate in this system but not for the utilization of formaldehyde. The role of homocysteine in these systems has not been elucidated.

The relationship of CFX to the active cofactor is under investigation. By analogy with other cofactors, the functional form of the vitamin may be a nucleotide or dinucleotide. A pentose substituent at the 8-position of the reduced pteridine, as suggested by Welch and Heinle (*29*), would tend to stabilize the structure and this would make lability to oxygen less likely. By analogy with the structure of coenzyme A, the possible attachment of a phosphate substituent through the essential hydroxyl group at the 4-position should also be considered. CFX itself may be a derivative of the cofactor, since it was found in high concentration in the medium suspending the intact cells, while the enzymatic capacity to alter PGA was retained within the cells. The ability of PGA, CF, anhydroleucovorin, or tetrahydro-PGA to activate certain metabolic systems (*5–8, 30*) may reflect degrees of refinement of such systems with respect to the capacity to form the functional derivative. The relationship of the polyglutamate pteridines to the active forms of the vitamin remains to be clarified. It is probable that the CF-series of compounds will be extended to include a group of polyglutamates that could include CFX or more complex forms.

Our present studies (*31*) represent, in part, attempts to contribute to an explanation of the mechanism of resistance to folic acid antagonists in leukemia. It is apparent that experiments designed to investigate the manner by which a cell or organism circumvents the action of these toxic compounds must be concerned with the true product of the reactions blocked by these antagonists. The evidence that relatively stable CF and other folic acid-like compounds result from the nonenzymatic degradation of the metabolically formed product of the reactions susceptible to inhibition directs attention to the limitations of present techniques and alters the design of future experiments. The interrelationships indicated in Fig. 1 represent our current working hypothesis.

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* Scholar in cancer research of the American Cancer Society.

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Frederick D. Heald, Dynamic Plant Pathologist

FREDERICK DEFOREST HEALD, plant pathologist, mycologist, botanist, and in all a scientist, died in Spokane, Washington, 24 April 1954, at the age of 82. He had been professor emeritus since his retirement in 1941 from the positions of professor and head of the department of plant pathology in the College of Agriculture, and plant pathologist and head of the division of plant pathology in the Agricultural Experiment Station at the State College of Washington. Although he was not very active after his retirement, he certainly retired with an enviable record of professional achievement that had long since won him international renown.

Dr. Heald was born on 23 July 1872 at Midland City, Mich., the son of Henry Francis Heald and Hettie Charles Heald. After attending preparatory school, he entered the University of Wisconsin, where he received his B.S. and M.S. degrees. Following an additional year of intensive graduate study in plant physiology and plant pathology at the University of Leipzig, he received his Ph.D. degree in 1897.

His first professional position was professor of biology at Parsons College, Fairfield, Iowa, which he held from 1897 to 1903. In 1903 he went to the University of Nebraska, where he served successively as "adjutant professor" of plant physiology, as associate professor of botany and botanist of the Agricultural Experiment Station, and finally as professor of agricultural botany. In 1908 Dr. Heald moved to the University of Texas as head of the School of Botany, where he remained until 1912 when he went to work for the state of Pennsylvania and the U.S. Department of Agriculture as plant pathologist, investigating biological factors involved in the serious chestnut blight epiphytotic. His contributions to the knowledge of the life-history of the pathogen and the means of spread of the disease gained him a reputation as a very capable and dynamic plant pathologist and undoubtedly led to his appointment in 1915 as professor of plant pathology at the State College of Washington and plant pathologist in the Washington Agricultural Experiment Station.

By this time Dr. Heald was thoroughly convinced of the importance of plant pathology and was so enthusiastic about its bright future that he was able to persuade the college administration to create a separate department (from botany). This was accomplished in 1918, and it is entirely logical that this tremendously energetic and courageous man was promptly installed as head of the new department.

In 1899 Dr. Heald married Nellie Townley. Much of his outstanding professional development can be attributed to the constructive and balancing influence of this brilliant woman who was as ambitious for her husband's professional development as he was himself. From this marriage came three children, Doris (Mrs. A. H. Tonge), Henry and Marion (Mrs. Emil Shebesta). Mrs. Heald died in 1939. Her death left a great void in Dr. Heald's life. In 1942 he married Charlotte Chamberlin. She and all three children survive him.

Dr. Heald's professional influence has been pronounced through accomplishments in research, teaching, and scholarly writing. In addition, the personality and drive of the man himself has had no small influence on his students and associates.

Although Dr. Heald is best known for his contributions to plant pathology, his first love was expressed in plant physiology and in the biology and taxonomy of mosses. His earlier papers dealt with such diverse subjects as the histology of pulvini, regeneration in mosses, electric conductivity of plant juices, biology wall charts, and analytic keys to North American mosses. His first contribution in plant pathology, "Methods of investigating plant diseases," came in 1905. From this point on, his papers dealt almost exclusively with plant pathology and mycology. Following his appointment at the State College

of Washington, two fields of interest predominated in his publications: cereal smuts and storage decays of pears and apples. In 1926 the *Manual of Plant Diseases* was published. This book served a very great need and quickly became the plant pathologist's "bible" because it was a scholarly and exhaustive reference book for so many plant diseases. It has been used in many different parts of the world. Yielding to the demand for a shorter version more adapted to single-term or single-semester courses in plant pathology, Dr. Heald wrote the *Introduction to Plant Pathology*. In 1941 he collaborated with C. S. Holton in the publication of *Bunt or Stinking Smut of Wheat (a World Problem)*. Besides his books, Dr. Heald was author or coauthor of more than 120 scientific papers, bulletins, and so forth, during a period of 45 years. He also served as special editor for edition 2 of *Webster's New International Dictionary*, laboring evenings for several years preparing definitions of phytopathological and mycological terms.

Dr. Heald was a member of various scientific and professional societies. He was president of the American Microscopical Society in 1912 and of the American Phytopathological Society in 1932. He was associate editor of *Phytopathology* for 8 years. He built up an unusually fine private library, for he subscribed to many periodicals, bought essentially every book that appeared on botany, mycology, and plant pathology, and exchanged reprints extensively. In 1944 he officially gave this library to the department he had founded and built.

Just as his papers and books testify to unusual achievement in research and writing, so do Dr. Heald's students testify to the man's ability as a teacher. How many undergraduate majors in plant pathology were students of Dr. Heald is not known, nor exactly the number of graduate students, but at the State College of Washington alone 15 graduate students have received the M.S. degree and seven the Ph.D. under his direction. These students are well distributed in responsible positions all over the country, and their performance has attested the high quality of the teaching and direction they received.

Dr. Heald's students are impressively in agreement on his ability as a teacher. As one of them phrased it,

> After instilling in his students true appreciation for fundamentals in science and a desire for accuracy, he guided by gentle urging and correction, which allowed them to learn as well from their errors as from adherence to his directions and suggestions. Attacks by others upon his students or his staff he met with a ferocity of defense which usually carried the day and certainly was never forgotten.

That teaching came naturally to him is witnessed by further testimony:

> I doubt that one could be blessed with a more natural and inspiring teacher.
>
> He was so skilled an instructor, we did not know we were being taught. He was a very inspiring teacher who made the subject of plant pathology alive, fascinating, and interesting.

Dr. Heald was also more than merely a teacher to his students. He took a strong personal interest in them.

> Dr. Heald was a father as well as a teacher. He was always fighting for the good of his students even to the point of dictating to his superiors.
>
> He had complete generosity, natural kindness and good nature [with his students], but he was on occasion as critical and adamant with his colleagues and superiors as he was understanding and forbearing with his students.

Dr. Heald was also a very active man physically. He owned a small acreage near the campus and in his earlier days at Pullman he would often arise at 5 A.M. to work in his garden before going to his office at 8 A.M. Then he would have a session of tennis or volley ball in the late afternoon and work on his writing until 10 or 11 P.M. In all of his athletic activities he played to win and he disliked losing. He played in summer school tennis tournaments even when he was in his mid-sixties. On his 60th birthday he wrestled, and threw, a much younger and somewhat larger colleague (in a friendly spirit, of course).

The man also had a pronounced sense of humor, specializing in puns. He could tell and enjoy good jokes with the best of them.

I take this occasion to express my gratitude for the kindly personal interest, the patient forbearance, and the multiplicity of courtesies accorded me by Dr. Heald when I began my own professional career in 1934 under his direction, and during the ensuing years as well. I wish to acknowledge the help of members of Dr. Heald's family for data concerning his earlier life, and of his earlier students (that is, prior to my association) for testimony about his effectiveness as a teacher.

GEORGE W. FISCHER

Department of Plant Pathology,
State College of Washington, Pullman

What we need is not the will to believe, but the wish to find out, which is the exact opposite.—BERTRAND RUSSELL.

News and Notes

Annual Meeting of the AAPT

The 24th annual meeting of the American Association of Physics Teachers was held at the Hotel McAlpin in New York, 27–29 Jan., concurrently with the annual meeting of the American Physical Society.

Contributed papers occupied two sessions, and many excellent papers were presented. One of the most stimulating was "An experimental encounter with the equation of time" by Richard M. Sutton (Haverford College), and the most entertaining was "Demonstration experiments" by Eric M. Rogers (Princeton University).

One day was devoted to a somewhat pressing topic continued from a year ago, namely, the role of physics in engineering education. Discussion at the morning session was based on reports from six eastern universities of recent intramural conferences between their departments of engineering and their departments of physics and at the afternoon session on reports by both engineers and physicists of last year's conferences of the Society for Engineering Education, in particular, the ASEE recommendation that solid-state physics and nuclear physics be included in the engineering curriculum. The primary purpose was to acquaint the membership with what is being done and to get reactions. One came away with a feeling of encouragement that the question of what physics can and should contribute to engineering education is at last being faced squarely by physicists and engineers throughout the country and that they are making good progress toward understanding each other's viewpoints and settling differences in the interest of better engineering education.

The long-standing problem of physics in the secondary curriculum was the subject of a panel discussion under the following headings: report of the Berkeley meeting of AAAS on science teaching, the role of science in the secondary school, problems of the high-school teacher and the future outlook for science teachers. A symposium on audio-visual aids covered special methods in physics lecture demonstrations, science demonstrations on television, and the production of animated films in physics. A new film on stationary longitudinal waves and one on the properties of liquid helium were shown.

At the traditional joint session of AAPT and APS, the Oersted medal for notable contributions to the teaching of physics was presented to Vernet E. Eaton (Wesleyan University, Connecticut). The 13th Richtmeyer memorial lecture was delivered by Eugene P. Wigner (Princeton University), whose subject was "On the development of the compound nucleus model."

At the annual business meeting, special citations for meritorious service to the association were voted to William S. Webb (University of Kentucky) and Mark W. Zemansky (City College of New York). Officers for the coming year are president, R. Ronald Palmer (Beloit College); president-elect, Walter C. Michels (Bryn Mawr); secretary, Frank Verbrugge (Carleton College); treasurer, Francis W. Sears (M.I.T.).

Concurrent meetings of AAPT and ASEE will be held at Pennsylvania State University, 21–23 June.

AUSTIN J. O'LEARY

City College of New York

Science News

During the Joint Atomic Energy Committee hearings 31 Jan.–3 Feb. on the state of the atomic energy industry, the resignation of Kenneth D. Nichols, general manager of the Atomic Energy Commission, was discussed. Two senators cited this resignation as an example of **"raiding" by private industry**; then Sen. Albert Gore (D., Tenn.) said that his office is considering the introduction of a bill that would bar an engineer, for 3 yr after his resignation, from working for a private firm on any project with which he had been connected while in Federal service. There is a similar law in effect that applies to Government lawyers, but the ban runs for only 2 yr.

Lewis L. Strauss, chairman of the AEC, stated that he would favor a bill such as Gore described; Commissioner Thomas E. Murray would not. Murray stated that such a law would have most unfortunate effects on the effort to recruit AEC personnel. Gore still has the bill under consideration.

Last summer the 18 nations on the United Nations Economic and Social Council unanimously adopted a resolution introduced by the Government of India asking all governments, whether U.N. members or not, to study the problem of calendar reform and present their views by May 1955. The calendar that is under consideration is the 12-mo, equal-quarter plan called the **World Calendar**. The AAAS Council endorsed the World Calendar in 1935 and in 1954 the Board of Directors reaffirmed this endorsement. The material that follows has been taken from the *Journal of Calendar Reform* that is published by the World Calendar Association, Inc., New York.

> In this proposed calendar every year is exactly the same; 1 January is always a Sunday. Each month has twenty-six weekdays plus Sundays. January, April, July, and October, the first months of each quarter, have thirty-one days; the other months thirty. The calendar is stabilized by ending the year with a 365th day which follows 30 December and is dated W, or Worldsday, a world holiday. (Leap-Year Day is similarly added after 30 June every fourth year, and becomes another world holiday.)
>
> The World Calendar is a workable synthesis of many proposals. Its key feature reaches back to 1834 when an Italian priest, Abate Marco Mastrofini, was struggling with the puzzle of how to fit

weeks, months, quarters, and half-years into a year of 365 days. Since 365 is divisible only by five, the puzzle had stumped the experts. But Mastrofini hit upon an ingenious solution: take the 365th day out of its weekday sequence, treat it as an extra holiday without a weekday name, and thus obtain, in effect, a year of 364 days, which can be divided evenly into fifty-two weeks. By the simple device of having one "blank" day any date of a given month will fall on the same day of the week every year.

In 1952 Prime Minister Nehru named a calendar reform committee, and early in 1953 he publicly stated that today's calendar, introduced by Caesar in 45 B.C., and readjusted by Pope Gregory in 1582, "has defects which make it unsatisfactory for universal use." Then in the fall of 1953 the Government of India proposed to the U.N. the adoption of The World Calendar. India officially stated this reform would overcome the "drawbacks of the present Gregorian Calendar. It is scientific, uniform, stable, and perpetual. It offers harmony and order to all strata of society—government, finance, industry, labor, retail trade, administration of justice, home life, transportation, and education."

Civil defense exercises and demonstrations at one of the **atomic explosions** in the 1955 spring test series in Nevada will be witnessed by hundreds of invited civilian observers. The series started in mid-February. The "open shot" for civil defense exercises and demonstrations has been scheduled tentatively for mid-April and Lewis L. Strauss, chairman of the Atomic Energy Commission, has stated that the date will be announced about 1 mo in advance. He warned, however, that indefinite postponement is possible if weather conditions at the scheduled time are not suitable for the detonation.

The Federal Civil Defense Administration will invite state governors, as well as city and state civil defense officials and other observers, to witness the exercise. Representatives of press, radio, television, and motion pictures will also be invited. The technical program to be held in conjunction with the open shot will be conducted jointly by FCDA and private industry. Observers will be able to inspect some test structures before and after the shot.

A report on what appears to be the cause of a **widespread rice disease,** known in Java, Malaya, Ceylon, India, and Burma, has been published in the 5 Feb. issue of *Nature* by F. N. Ponnamperuma, R. Bradfield, and M. Peech, agronomists of Cornell University. Browning of leaves and roots was observed in rice grown in greenhouse tests; this was traced to too much iron. No disease organisms were found to account for the effect, which was similar to that caused by potassium deficiency.

Cycloserine, a new broad-spectrum antibiotic that was discovered by Roger Harned and Elinor Kropp, microbiologists at the laboratories of the Commercial Solvents Corp., Terre Haute, Ind., has been found, in preliminary tests, to be **effective against tuberculosis.** Plans for putting the new drug on the market depend on the results of the further studies that will be conducted beginning 1 Mar. by the Veterans Administration as part of its chemotherapy program.

Cycloserine, which has been given the trade name Seromycin, has been administered orally to 37 tubercular patients at New York's Metropolitan Hospital since last September. Twenty-nine of these patients were chronic cases that had not responded to treatments with other antibiotics during a year or more. Thirty-six patients showed clinical improvement; x-rays indicated some improvement in infected lung areas in 28 patients; 30 gained from 4 to 14 lb during a 16-wk period, and their fever was reduced; simple smears of sputum and gastric concentrates, previously positive for all 37 patients, became negative in 30.

These preliminary results were reported by Israel G. Epstein, K. G. S. Nair, and Linn J. Boyd of New York Medical College, on 9 Feb. at the 14th VA-Army-Navy conference on tuberculosis in Atlanta, Ga., which was held in cooperation with the National Tuberculosis Association.

Cycloserine was also tried on infections of the genitourinary tract by George R. Nagamatasu and Lois Lillick of New York Medical College and Russell D. Herrold of the University of Illinois College of Medicine. They got good results in 46 of 62 stubborn infections that had resisted all other treatment. These last results were summarized at the Atlanta meeting by Henry Welch of the U.S. Food and Drug Administration, Washington, D.C., who also reported his own test-tube studies of cycloserine.

The development of an atomic "clock," the **Cesium Atomic Frequency Standard,** has been announced by Jerrold R. Zacharias, director of the Laboratory for Nuclear Science at Massachusetts Institute of Technology. He is working on a model so precise that if it had been ticking away since the time of Christ it would now be inaccurate by only ½ sec. Manufacture of a commercial model of the clock has been undertaken by a company in the Boston area and it will be available for many uses within a year.

Associated with Zacharias in the work have been James G. Yates, an English electrical engineer who was a visiting professor at M.I.T. and is now at Trinity College, and Robert D. Haun, a research assistant in the department of physics at M.I.T. The device was developed in the research laboratory of electronics with support in part from the Signal Corps, the Office of Scientific Research, the Air Research and Development Command, and the Office of Naval Research. Zacharias will give technical details of the clock at a meeting of the Institute of Radio Engineers on 23 Mar.

Time-keeping in the device is controlled by the oscillation of electrons in the cesium atom. As an electron revolves around the nucleus of an atom, it "wobbles" very slightly but at a constant rate. This unvarying rate of oscillation is reflected in the frequency of waves that are emitted.

Cesium is an element that has a frequency of approximately 9192.632 Mcy/sec, and it is this fre-

quency that serves as the unit of time in the clock. A metal crucible encloses 0.01 g of cesium. When heated to a temperature of about 100°C the cesium shoots a stream of atoms through a hole in the crucible. The atoms strike a detector screen, and their frequency is reported through a complex apparatus. The cesium emits atoms at a rate of about 1 million per second, but the loss is only about 1 μg/day and, for all practical purposes, the clock is perpetual.

Standard time, with 1 sec equal to 1/86,400 of the mean solar day and generally measured by the frequency of the oscillations of a crystal, is accurate to 10^{-3} μsec. The Atomic Frequency Standard is accurate to 10^{-4} μsec. Zacharias expects, through further development, to obtain accuracy of 10^{-6} μsec. The Cesium Atomic Frequency Standard is similar to the "maser," microwave amplification by stimulated emission of radiation, developed recently at Columbia University but operates on a different principle.

Hearings were begun 31 Jan. on H.R. 3005 to extend the **Selective Service authority to induct.** In order to support the position that Selective Service is no longer selective and that it should be returned to operation on the basis of selection, the Engineering Manpower Commission and the Scientific Manpower Commission request information on specific case histories that appear to demonstrate the abandonment of the criterions essential to selection. Cases in which all appeal procedures were exercised would be particularly useful. The EMC and the SMC would prefer to be able to use the information as is, but realize that many organizations are understandably reluctant to involve their companies and/or specific employees in such discussion. Upon request, therefore, the material will be used without identification of source. Data should be forwarded to W. T. Cavanaugh, Executive Secretary, Engineering Manpower Commission of Engineers Joint Council, 29 W. 39 St., New York 18.

The Spanish Ministry of Agriculture has announced plans to **irrigate 1¼ million acres** of unproductive land at the rate of 125,000 acres/yr. The work will be carried out by the National Institute of Colonization, partly with United States equipment granted to Spain by the Foreign Operations Administration. The institute, directed by agronomist Angel Martinez Borque, was founded in 1939 for the purpose of improving and reclaiming land to increase production. It is empowered by law to expropriate land. The institute permits owners of expropriated property to retain between 50 and 250 acres; the rest is purchased by the institute at the prevailing market value, which is assessed on the annual yield. The new 5- to 10-acre farms that are created are sold to agricultural laborers on moderate terms.

A new chicken, the **Beltsville broiler,** which grows from a chick to a 3-lb bird in 10 wk, has been developed by poultrymen at the U.S. Department of Agriculture's Beltsville, Md., research center. The result of 7 yr of experimental work, the broiler is a Silver Cornish–New Hampshire cross. Geneticists responsible for the new bird emphasize, however, that the Beltsville broiler is not a breed. They explain that it must be produced by mating the new Silver Cornish, also developed at Beltsville, and New Hampshire stock. The best characteristics of these parents are reproduced only in the first generation. Although further work is needed with the breeding, the demand for the new broiler is already far greater than the supply. When available, limited numbers of eggs are sold to breeders and distributed to cooperating state experiment stations.

Thomas Francis, Jr., director of the **Polio Vaccine Evaluation Center** at the University of Michigan, has issued the following statement concerning the security measures taken in connection with the forthcoming vaccine report.

> Only the Vaccine Evaluation Center receives all of the requisite data from all the Vaccine Field Trial Areas. These reports from all areas are subjected to uniform standards of interpretation, employing the same criteria throughout for the final diagnoses and classification of cases reported to be poliomyelitis.
>
> It was agreed by everybody officially concerned with the Field Trial that no preliminary guesses or fragmentary estimates of results would be made by any agency prior to the report of the total results by the Vaccine Evaluation Center; this agreement is still firmly in effect.
>
> For security reasons the preparation of material for analysis is handled in such a manner that no person has more than a limited view of the data. The transcribing of information concerning cases to be entered on punch cards is done in code with different persons preparing the various sections of the data: The Clinical and Epidemiological Reports, the Muscle Examinations and the Laboratory Reports. Diagnostic classification will be made without reference to the vaccination status of the patient, so long as he is a member of the total study population. Advisory consultation on any phase of the work is concerned only with establishing criteria for analysis, not with results.
>
> Thus, when the various investigations leading to final diagnoses of a case are completed, the data are compiled in a series of codes which would be impossible to interpret without the proper keys. It has not been the practice to prepare preliminary evaluations at repeated intervals, since attention and effort have been necessarily focused upon the essential job of obtaining complete and accurate reports which will permit a reliable diagnosis of poliomyelitis and paralysis.
>
> When analyses are made, they are handled entirely by certain designated members of the senior staff of the Center so that the information cannot be available to others. There is, hence, little likelihood that significant information can be gained casually. It is even more unlikely that the professional staff of the Vaccine Evaluation Center will be communicative, except when the report is made. Due provision is made for maintaining this restrictive security throughout the period of preparation of the report and its presentation. When the results are presented, it is intended that they will be made available to all parties at the same time.

Scientists in the News

Albert Szent-Gyorgyi, Hungarian-born winner of the 1937 Nobel prize in medicine who is now director of the Institute for Muscle Research at Woods Hole, Mass., was honored 10 Feb. at a dinner at the New York Academy of Sciences for his contributions to humanity. The dinner preceded a daylong conference on "Bioflavonoids and the capillary" at which all the work reported had its genesis in Szent-Gyorgyi's discoveries in the 1930's. Szent-Gyorgyi then isolated hesperidin as the factor that controls capillary function. It was later learned that ascorbic acid also has to be present in order that hesperidin exert its influence upon capillaries. More than 500 professional papers have since been written on the subject to which these discoveries provided the key. Szent-Gyorgyi presided over the meeting and in addition was presented with a citation dedicating the conference to him.

Paul J. Kramer, professor of botany at Duke University, will spend 6 to 8 wk this spring at the Earhart Plant Research Laboratory of California Institute of Technology, where he will study the effects of different photoperiods and day and night temperatures on the distribution and length of growing season of certain tree species. The series of experiments, supported by a National Science Foundation grant, will continue for 3 yr at both the Earhart Laboratory and Duke University under Kramer's direction.

Dennis Sikes, former professor of veterinary science at the University of Tennessee, joined the faculty of the School of Veterinary Medicine, University of Georgia, on 1 Feb. as professor and head of the department of pathology and parasitology.

Leslie C. Dunn, director of the Institute for the Study of Human Variation, Columbia University, will deliver the 1955 Westbrook free lectureship on the subject of "Heredity and the human community" at the Wagner Free Institute of Science in Philadelphia on 14, 21, and 28 Apr. Individual topics to be discussed on these dates are "The essence of heredity," "The community as viewed by a biologist," and "The social direction of evolution."

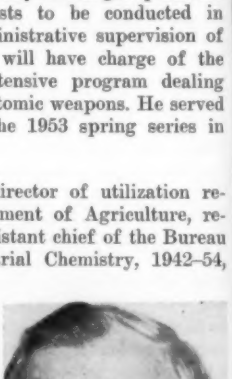
Two visiting professors have been named this term in the division of the geologic sciences at California Institute of Technology. **Felix Chayes,** petrologist in the Geophysical Laboratory of Carnegie Institution, has been appointed visiting professor of petrology. **William W. Rubey,** geologist for the U.S. Geological Survey and chairman of the division of geology and geography of the National Research Council, is filling a 6-mo appointment as visiting professor of geology. He is known for his work on fluid mechanics as applied to the geologic processes of running water, deposition, and sedimentation as well as for his studies of the origin and evolution of the oceans.

Gordon Millichap, associate professor of pediatrics, St. Bartholomew's Hospital Medical College, London, is conducting research in the department of pharmacology at the University of Utah College of Medicine under the auspices of a British Medical Research Council fellowship.

Aldo P. Truant, former associate professor of pharmacology at Tufts Medical and Dental College, has joined the staff of a new research laboratory, Astra Pharmaceutical Products, Inc., Worcester, Mass., as director of biological research. The laboratory will be in operation by 1 May.

Donald A. Quarles, Assistant Secretary of Defense for Research, has appointed **Edward B. Doll,** chairman of the physics department at Stanford Research Institute, director of the military effects group for the spring series of atomic tests to be conducted in Nevada. In addition to administrative supervision of the group's activities, Doll will have charge of the scientific aspects of an extensive program dealing with the military effects of atomic weapons. He served in a similar capacity for the 1953 spring series in Nevada.

Carl F. Speh, assistant director of utilization research for the U.S. Department of Agriculture, retired on 31 Jan. First as assistant chief of the Bureau of Agricultural and Industrial Chemistry, 1942–54, and later as assistant director of utilization research, he was responsible for the development, coordination, amplification, and refinement of the bureau's program of utilization research on cereal and forage crops, sugar, naval stores, tobacco, tanning materials, and wild plants carried on at the four large regional laboratories of the USDA.



Speh graduated from Yale University with the Ph.D. degree in chemistry in 1906. He later did 2 yr of postgraduate work in organic chemistry at the same institution. His first job was as a chemist with the U.S. Forest Service in Washington, and it was here that he began his research on naval stores. From 1908 to 1916 he carried on the same type of work at the department's Bureau of Chemistry, where his responsibilities included analyses of turpentine and rosin to establish their properties and determine their suitability for various uses, work on the distillation of pine wood, and the establishment of official United States rosin standards.

From 1916 to 1927 Speh was secretary-manager of the Turpentine and Rosin Producers Association and

concurrently, 1925 to 27, secretary-treasurer of the Naval Stores Export Corporation, which he organized under the Webb-Pomerene Act. From 1927 to 1934 he was secretary-manager of the Pine Institute of America, and from 1934 to 1936, secretary of the A.A.A. Gum Control Committee. His work during this period covered the planning and inauguration of all chemical and industrial research on naval stores, initiation of a timber conservation program, supervision of the research fellowship at Mellon Institute, and development of new uses for gum rosin and gum turpentine.

Speh returned to Government service in 1936 as senior technologist in the naval stores research division of the Bureau of Agricultural and Industrial Chemistry. In 1938 he was made chief of that division, a position he held until he became assistant chief of the bureau in 1942. He served in this capacity until the reorganization of the USDA in January 1954 when he became assistant director of utilization research.

Charles E. Wilson, president of the board of scientific directors of the Roscoe B. Jackson Memorial Laboratory, Bar Harbor, Me., has announced three new elections to the board: **Charles H. Best,** director of the Banting and Best department of medical research at the University of Toronto; **Walter E. Heston,** chief geneticist of the National Cancer Institute, Bethesda, Md.; and **John G. Kidd,** pathologist-in-chief of the New York Hospital and Cornell Medical College. Other members of the board in addition to Wilson, who is vice president of the National Academy of Sciences and professor emeritus of the Harvard School of Public Health, are Frank A. Beach, department of psychology, Yale University; Clarence Cook Little, founder and director of the Jackson Laboratory; H. B. Andervont, senior biologist, National Cancer Institute; Leonard Carmichael, secretary, Smithsonian Institution; L. C. Dunn, professor of zoology, Columbia University; John J. Morton, professor of surgery, School of Medicine and Dentistry, University of Rochester; Richard E. Shope, member, Rockefeller Institute for Medical Research.

Gerhard Herzberg, director of the division of physics of the National Research Council of Canada, has been elected an honorary fellow of the Indian Academy of Sciences "in recognition of his scientific eminence and of his outstanding contributions to knowledge."

During a sabbatical leave, and with the assistance of a U.S. Public Health Service grant, **William F. Diller,** associate professor of zoology at the University of Pennsylvania, will undertake cytological studies of protozoa at the department of biology, University of Mysore, Bangalore, India.

Under the auspices of the Foreign Operations Administration and the U.S. Department of Labor's Bureau of Apprenticeship, **N. R. Kuloor** has joined Foster D. Snell, Inc., consulting chemists and engineers, as a trainee chemical engineer. Kuloor, who will be with Snell about 4 mo, is on leave of absence from the Shri Ram Institute for Industrial Research in New Delhi, India, where he has worked as chief chemical engineer for more than 7 yr on the design and fabrication of pilot plants. The Shri Ram Institute is a privately endowed foundation that provides assistance to industry on technical problems and performs applied research.

Marshall C. Harrington, professor of physics at Drew University, has resumed his teaching duties after a 2½-yr absence on a UNESCO technical assistance mission in Baghdad, Iraq. During his stay he was concerned with developing facilities, planning the curriculum, and recruiting teachers for the physics department of the College of Arts and Sciences; in his first year in Iraq he also taught at the college. Following the completion of his UNESCO assignment he traveled in several European countries, and then spent a sabbatical leave visiting various English universities to observe programs of undergraduate laboratory work in physics.

Ralph B. Houlihan, who has been associated with Cutter Laboratories, Berkeley, Calif., since 1946, has been appointed associate director of research. During the past year, as director of biological research, he has been in charge of the production of the Salk-type polio vaccine.

Necrology

Raymond Asserson, 64, former assistant chief engineer for the Federal Communications Commission, Washington, D.C., 3 Feb.; **Moses N. Baker,** 91, retired editor of *Engineering News,* Upper Montclair, N.J., 7 Feb.; **William E. Galt,** 50, psychologist, author, research associate for the Lifwynn Foundation, Westport, Conn., 5 Feb.; **Theodore J. Hoover,** 84, mining engineer, author, fish and game conservationist, dean emeritus of the Stanford University School of Engineering, Stanford, Calif., 4 Feb.; **Hugh Keeling,** 89, chief engineer in the building of New Delhi, India, 3 Feb.; **Joseph B. Kincer,** 80, agricultural meteorologist, retired head of the Climatological Services Div. of the U.S. Weather Bureau, Washington, D.C., 14 Dec.

Adolph Magnus-Levy, 90, physiologist, biochemist, pioneer in the fields of metabolism and diabetes, New York, 6 Feb.; **Phillip H. Mitchell,** 71, biochemist, physiologist, author, former professor and head of the biology department at Brown University, Providence, R.I., 2 Feb.; **Charles J. Pannill,** 75, pioneer in the development of radio communications, president of the Radiomarine Corp. of America and R.C.A. Institutes, New York, 7 Feb.; **Edward Reiter,** 59, oral surgeon, lecturer, Cleveland, Ohio, 5 Feb.; **Queenie H. Shirley,** 42, electrical engineer, instructor in physics and engineering at Assumption College, Windsor, Ont., 1 Feb.; **Charles B. Sloane,** 60, profes-

sor of chemistry at Seton Hall University, South Orange, N.J., 3 Feb.; **Norman Strauss,** 54, gastroenterologist, author, former associate professor of medicine at New York Medical College, New York, 6 Feb.; **Frederic A. Woll,** 80, optometrist, author, professor emeritus and retired chairman of the hygiene department of City College, New York, 5 Feb.

Meetings

The **New Jersey Academy of Science** held its annual meeting in Chester on 31 Jan., during which the membership adopted a constitution and bylaws. The aims of the organization are

> To stimulate scientific education and research and the diffusion of scientific knowledge in the various departments of science; to promote fraternal relationship among those engaged in scientific work; to assist in the development [of] and in making known the material and other resources of the State; to publish the reports of scientific investigations; to unify the scientific interests of the State; to encourage interdisciplinary study and research; and to investigate and report on any subject of science or industry, when called upon by any department of the State government of New Jersey.

Papers were presented by Hirsch L. Silverman, Robert Zuck, and Michael Charney. The following officers were elected: pres., Roger H. Charlier; v. pres., Robert Zuck; sec., Courtland J. Daley, director of the audio-visual aids department of Cranford High School; treas., Hirsch L. Silverman.

The second **Microcirculatory Conference for Physiology and Pathology,** sponsored by the American Association of Anatomists, will take place 5 Apr. in the Benjamin Franklin Hotel, Philadelphia.

The feasibility of **consultation by pathologists through the medium of color television** was explored for the first time by a 3-day symposium sponsored by the Armed Forces Institute of Pathology at its new building in northwest Washington. The meeting, which began on 17 Jan., was climaxed by a color television presentation that brought pathologists from different cities together for consultation. In Philadelphia, at the hospital of the University of Pennsylvania, a surgeon performed an operation. A waiting pathologist there carried out an examination of the tissue removed; TV cameras then transmitted the picture to pathologists in Baltimore and Washington who assisted in the diagnosis.

At the AFIP symposium, also for the first time, representatives from medicine and industry discussed how the newest communications medium can best be used in medical education and in the diagnosis and treatment of disease. Although color television has been used for several years in medicine and surgery, no evaluations have been made of this medium in the field of pathology.

Education

Forty teenagers—eight girls and 32 boys—were named as finalists in the 14th annual **Science Talent Search,** a competition that began with 16,033 aspirants in high schools in all 48 states and the District of Columbia. Begun in 1942, the Science Talent Search is conducted by Science Clubs of America through Science Service. The awards are made by the Westinghouse Educational Foundation, which is supported by the Westinghouse Electric Corp.

Named by a committee of judges as the nation's most promising future scientists, the 40 students were awarded all-expense trips to Washington and a chance to compete for $11,000 in Westinghouse science scholarships. They arrived in Washington 24 Feb. to attend the annual 5-day Science Talent Institute, during which the winners were chosen and scholarships were awarded. The finalists were all graduating seniors from high schools in 17 states.

The University of Texas Medical Branch in cooperation with the Texas Academy of Science is presenting a series of 16 weekly television broadcasts, entitled ***Man and Medicine,*** over KPRC in Houston. The program is given at Sunday noon.

The new 325-bed, $6.5 million St. Vincent Hospital in Little Rock, Ark., operated by the Sisters of Charity of Nazareth, was dedicated recently. Fifteen rooms have been allotted to the laboratory section, which is connected with an accredited **School of Medical Technology.** This school has been in existence since 1953 and has just recently graduated its first students. In July 1954, the school was given a grade of 98 percent by the American Council of Medical Education.

The High Voltage Engineering Corp., of Cambridge, Mass., will supply all the vital components of a 6-Mev Van de Graaff accelerator soon to be assembled at **Imperial College,** Kensington, England. The machine will be used in a program of fundamental nuclear research conducted by Samuel Devons, professor in the college.

Construction has begun at **Columbia University** on a new physics research building to be known as the **George B. Pegram Laboratory.** Scheduled to be completed in August, the building's cost is estimated at $350,000, approximately $295,000 of which is being provided by the Atomic Energy Commission. The remainder will come from the income from a special bequest to the university that is available only for scientific purposes. The AEC will also provide the laboratory with a new 6-Mev Van de Graaff generator. Unlike the building, which becomes the property of Columbia, the generator will be on indefinite loan and will remain the property of the AEC. The machine is being made by the High Voltage Engineering Corp., Cambridge, Mass. The cost of the generator, auxiliary equipment, and installation will be about $450,000.

The laboratory—which is named for Pegram, a nuclear physicist and at present special adviser to the president of Columbia—will stand next to the eastern end of the present Pupin Physics Laboratories building on 120th St. near Broadway. The main section of the three-story structure will measure 45 by 65 ft. A seven-story tower section will house the vertical accelerator and its metal shield.

The new Van de Graaff generator will bring to three the number of nuclear particle accelerators at Columbia. The other two are the 385-Mev cyclotron at Nevis in Irvington-on-Hudson, N.Y., and the "baby" cyclotron in Pupin laboratory, both of which produce beams at energy levels of from 10 to 20 Mev. The Van de Graaff machine will have the lowest power of the three, but its energy level will be readily adjustable up to a maximum of 6 Mev.

Educational television was expanded to 11 stations by the addition of three new stations during the first weeks of January. Transmitting their first programs within a few days of one another were KCTS, Seattle, Wash.; WEDM, Munford, Ala., and WUNC, Chapel Hill, N. C. The three new stations can reach a potential audience of about 5 million viewers, bringing the total population living within range of educational TV stations to nearly 20 million. Weekly schedules of the three new stations will soon total some 80 hr, raising the total weekly program output of educational TV to more than 250 hr. The nation's first statewide network entered its initial stage of active programing when Munford, main link in Alabama's educational TV network, started regular telecasting on 15 Jan.

Yale University's Edwards Street Laboratory, supported by the Office of Naval Research, is described in the February issue of the *Yale Alumni Magazine*. Details of the research under way there cannot be made public, but the laboratory came into being late in 1950 when the U.S. Navy suddenly realized that, because of an inadequate harbor defense, it had temporarily lost command of the sea in Korean waters. Louis W. McKeehan, professor of physics at Yale and a retired naval captain, [*Science* **121**, 54 (14 Jan. 1955)], persuaded both Yale and the Navy to join in a research and experimentation project dealing with harbor defense. McKeehan was director of the laboratory until July of last year, when he was succeeded by Andrew Patterson, Jr., associate professor of chemistry. Although he is a physical chemist, Patterson worked in the field of underwater sound during World War II and has served as a consultant on sonar and related topics at the Navy Underwater Sound Laboratory in New London, Conn. The Edwards Street Laboratory is reported to be the only laboratory in the country working on the problems of harbor defense.

All the activities of the laboratory, however, are not carried on in the Edwards Street building, which is for the most part occupied by offices, a rapidly growing library of books and scientific papers, calculating machines, a machine shop, and photographic equipment. Field studies directly involving submarine mines have been carried on at Yorktown, Va., Charleston, S.C., and New London, Conn., and much research in radar has been conducted in Narragansett Bay, R.I.

Regionalization of health resources and services in defined areas to maintain highest levels of medical care has been advanced as a solution of the problem of continuous postgraduate education. Such a plan was brought forward in 1949 when grants of $1.2 million were authorized under the Hospital Survey and Construction Act, but appropriation was halted because of the Korean War. In an article in the February issue of the **Journal of Medical Education**, John B. Grant of the Rockefeller Foundation division of medical and public health has outlined a plan that envisions an area hospital council, a system of independent hospitals integrated as one giant hospital with distant branches related as if they were wards or divisions of a large medical center. The purpose is to develop in the community hospital an approximation of the teaching hospital. Grant describes the administration, activities, and support of 12 selected programs.

The 14th annual **Frontiers in Chemistry lecture series** started at Western Reserve University on 25 Feb. with a presentation on "Recent advances in photochemistry" by Albert Noyes of the University of Rochester. The balance of the program is as follows: 4 Mar., "Chemistry of free radicals," M. S. Kharasch, University of Chicago; 11 Mar., "Radiation chemistry of liquids," Milton Burton, University of Notre Dame; 18 Mar., "Rearrangements in carbonium ion reactions of aliphatic and alicyclic compounds," John D. Roberts, California Institute of Technology; 25 Mar., "Effects of high-energy radiation on polymers," A. M. Bueche, General Electric Co.; 15 Apr., "Aromatic complexes as intermediates in substitution reactions," Herbert C. Brown, Purdue University; 8 Apr., "Radiation chemistry of inorganic solids," John A. Ghormley, Oak Ridge National Laboratory; 29 Apr., "Formation and behavior of intermediates from neighboring group participation," Saul Winstein; 22 Apr., "Paramagnetic resonance studies in radiation chemistry," Max S. Matheson, Argonne National Laboratory; 6 May, "Reaction mechanisms involving elemental sulfur," Paul D. Bartlett.

Color telecasts from an operating room at the Walter Reed Army Hospital were used for the first time by the Army for nursing instruction during the Army Nurse Corps Postgraduate Workshop on Military Operating Room Nursing held 14–25 Feb. at the Army Medical Service Graduate School, Walter Reed Army Medical Center. Workshop participants in another room both saw and heard an operating surgeon progress through the various steps of an operation.

Available Fellowships and Awards

Saint Louis University has been awarded an $8100 grant by E. I. du Pont de Nemours and Co. for the general support of its Institute for the Teaching of Chemistry. A $4500 fund will provide 12 **fellowships in chemistry** for high-school and junior-college teachers who wish to attend the institute during the summer. The fellowships provide tuition of $100 and living allowance of $180.

In addition $3600 will support two fellowships for college graduates who wish to work toward an M.S. degree in the teaching of chemistry. A year's study will prepare recipients of the fellowships to teach chemistry, physics, or mathematics in a secondary school. Each fellowship will provide $450 tuition and a stipend of $1200. Further information may be obtained from Dr. Theodore A. Ashford, Director of the Institute for the Teaching of Chemistry, Saint Louis University, St. Louis, Mo.

The annual **Aero-Corry research award** competition for graduate and undergraduate engineering students has been announced by the Aero Supply Mfg. Co., Inc. The new competition was established recently by Aero Supply to encourage research and design in the field of aircraft fuel systems. A first-place cash prize of $1000—then $500, $300, and $100—will be awarded for the best research papers dealing with (i) complete fuel system design, and (ii) fuel system component design.

Engineering schools are asked to encourage students to compete for the prizes. Circulars containing competition rules and fifteen sample problem statements are available. Among the suggested problem statements for possible research are "Ignition limits when hot air is used to transfer fuel" and "Behavior of fuel at temperatures below 20°F." Entries for the 1955 competition *must be postmarked no later than 30 Apr.* and should be addressed to Aero-Corry Research Award, Aero Supply Mfg. Co. Inc., Corry, Pa.

The **University of Texas M. D. Anderson Hospital and Tumor Institute** offers an extensive program of cancer fellowships and residencies that are under the auspices of the University of Texas Postgraduate School of Medicine. For information and application forms write to Grant Taylor, M.D., Office of Education, The University of Texas M. D. Anderson Hospital and Tumor Institute, Texas Medical Center, Houston 25.

To encourage the attainment of the highest standards of scientific reporting, the American Heart Association will present annual awards to the individuals whose creative efforts in any medium of mass communication are judged to have contributed most to public understanding of progress in research, and in the prevention, care, and treatment of heart and circulatory disease.

The deadline date for submission of entries for the 3rd annual **Howard W. Blakeslee** awards has been *extended until 1 May*. The number of awards and the media from which winners are to be selected will be determined by the judges; a minimum of $500 will be presented to the winner.

In order to be eligible for the 1955 awards, materials in the fields of press, magazines, books, radio, television, and films must have been published, issued, or produced between 1 Jan. 1954 and 1 Mar. 1955. Entry blanks and information may be obtained from the Chairman, Managing Committee, Howard W. Blakeslee Awards, American Heart Association, 44 E. 23 St., New York 10.

Grants and Fellowships Awarded

The **National Science Foundation** has announced 120 grants totalling approximately $1.6 million for the support of basic research in the natural sciences, for conferences and studies on science, for scientific information exchange, for the scientific manpower register, and for travel of American scientists to international scientific meetings. This is the second such group of awards to be made during fiscal 1955.

Harvard University. G. R. Willey, Peabody Museum. Prehistoric settlement patterns in the Maya area, 1 yr, $11,500.

Columbia University. J. Schilt, astronomy. Trigonometric parallaxes of stars, 1 yr, $2400.

Indiana University. J. B. Irwin, astronomy. Photoelastic observations of southern cepheids, 1 yr, $5800.

Brigham Young University. J. R. Goates, chemistry. Mechanism of adsorption of ions by silicate minerals, 1 yr, $5100.

University of California, W. F. Giauque, chemistry. Thermodynamic and magnetic properties of matter at low temperatures, 1 yr, $30,000.

Cornell University. J. Meinwald, chemistry. 1,3 shift in molecular rearrangements, 2 yr, $11,000.

Harvard University. G. B. Kistiakowsky, chemistry. Unstable intermediates in gas reactions, 2 yr, $25,400.

University of Illinois. B. R. Ray, chemistry. Transference numbers of salts in nonaqueous solvents, 6 mo, $3400.

Iowa State College of Agriculture and Mechanic Arts. G. S. Hammond, chemistry. Primary products of thermal decomposition reactions in solution, 2 yr, $12,900.

Johns Hopkins University. A. H. Corwin, chemistry. Synthetic studies on chlorophyll, 2 yr, $13,900.

Mellon Institute of Industrial Research. F. A. Miller, research in chemical physics. Relative energies of polar and equatorial derivatives of cyclohexane, 2 yr, $6200.

Northwestern University. A. A. Frost, chemistry. Molecular potential energies, 2 yr, $10,000.

University of Pittsburgh. J. L. Rosenberg, chemistry. Chemiluminescence of photosensitizing organic dyes in condensed systems, 3 yr, $10,700.

University of Rochester. W. D. Walters, chemistry. Kinetics and mechanism of thermal reactions, 3 yr, $16,000.

Smith College. M. D. Soffer, chemistry. Synthetic and structural investigations in the sesquiterpene series, 2 yr, $9000.

University of Washington. K. B. Wiberg, chemistry. Mechanisms of oxidation reactions, 3 yr, $17,000.

Wayne University. C. Djerassi, chemistry. Alpha-amyrin chemistry, 2 yr, $16,000.

University of Wisconsin. E. E. Van Tamelen, chemistry. Synthesis of alkaloids, 2 yr, $9000.

Duke University. C. G. Bookhout, Duke University Marine Laboratory. Reproduction, life histories, and metamorphosis of shrimp, 1 yr, $3500.

University of Chicago. H. Ramberg, geology. Thermodynamic study of minerals, 3 yr, $27,000.

Columbia University. J. L. Kulp, geology. Carbon-14 dating of archeological and anthropological specimens, 1 yr, $10,000.

Dartmouth College. J. B. Lyons, geology. Systematic compositional variation in metamorphic minerals, 2 yr, $7200.

Franklin and Marshall College. R. M. Foose, geology. Structural patterns around the perimeter of the Beartooth Block in southwestern Montana, 2 yr, $10,000.

Johns Hopkins University. R. B. Montgomery, oceanography. Analysis of serial oceanographic observations, 2 yr, $9000.

Lehigh University. H. R. Gault, Geology. Studies of carbonate rocks, 2 yr, $6800.

University of Minnesota. F. M. Swain, geology. Stratigraphy of bituminous deposits, 2 yr, $15,000.

Paleontological Research Foundation. K. V. W. Palmer, paleontological Research Institute. Molluscan fauna of the Ocala Limestone (Upper Eocene) of westcentral Florida, 2 yr, $11,900.

Pennsylvania State University. C. L. Hosler, College of Mineral Industries. Aggregation of ice crystals to form snow, 2 yr, $10,000.

Saint Louis University. R. R. Heinrich, geophysics and geophysical engineering. Atmospheric microoscillations and short-period microseisms, 3 yr, $36,000.

Texas A. & M. Research Foundation. D. W. Wood, oceanography. Calcium carbonate solubility equilibrium in sea water, 3 yr, $20,000.

Bureau of Reclamation, Dept. of the Interior. R. C. Mielenz, Design and Construction Div. Hydration of Portland Pozzolan Cement, 1 yr, $10,000.

California Institute of Technology. C. B. Millikan and H. W. Liepmann, aeronautics. Stability of fluid flow, 2 yr, $23,000.

Carnegie Institute of Technology. H. L. Toor, chemical engineering. Transfer of matter across gas-liquid interfaces, 2 yr, $13,500.

Georgia Institute of Technology. R. S. Ingols, Engineering Experiment Station. Protein changes with chlorine, 1 yr, $7100.

University of Illinois. M. E. Clark and O. M. Sidebottom, theoretical and applied mechanics. Inelastic behavior of columns, 1 yr, $8600.

Kansas State College. R. C. Hall, chemical engineering. Effect of sonic vibration on the rates of mass transfer, 2 yr, $12,000.

University of Maine. H. Gray, civil engineering. Moisture content and density of granular soils, 3 yr, $7900.

Massachusetts Institute of Technology. J. G. Trump, electrical engineering. Fundamental processes in high voltage discharges in vacuum, 2 yr, $18,700.

New York University. M. Telkes, Research Div. Solar energy collectors, 1 yr, $7000.

North Carolina State College. N. L. Nemerow, civil engineering. Mechanism of biochemical oxidation of organic matter, 1 yr, $7000.

Pennsylvania State University. W. E. Ranz, engineering research. Disintegration and dispersion of a liquid into droplets, 18 mo, $6300.

Pennsylvania State University. A. H. Waynick, electrical engineering. Ionosphere observations by long wave radio methods, 1 yr, $10,000.

Polytechnic Institute of Brooklyn. E. Weber, Microwave Research Institute. Electromagnetic networks and information-handling circuits, 1 yr, $10,000.

Princeton University. A. E. Sorenson, mechanical engineering. V. Olgyay and A. Olgyay, architecture. Thermal behavior of buildings by means of study of models, 2 yr, $19,100.

Swarthmore College. C. Barus, electrical engineering. Electronic instrumentation for neurophysiology, 1 yr, $7300.

University of California, Berkeley. C. L. Hubbs, Scripps Institution of Oceanography, La Jolla. Ecological conditions associated with eruptions of human populations, 3 yr, $25,200.

Duke University. P. J. Kramer, botany. Physiological processes of forest tree species, 3 yr, $18,900.

University of Notre Dame. A. L. Delisle, biology. Cytogenetic studies on the asters, 2 yr, $5000.

Union College. H. M. Butzel, Biology, W. B. Martin, Jr., chemistry. Mating type development and determination in paramecium, 2 yr, $8000.

Brown University. H. Federer and W. S. Massey, mathematics. Topology and measure theory, 30 mo, $36,500.

Carnegie Institute of Technology. A. Schild, mathematics. Relativistic particle mechanics, 2 yr, $8500.

Dartmouth College. J. G. Kemeny, mathematics. Mathematical methods in the behavioral sciences, 1 yr, $10,000.

Illinois Institute of Technology. G. Berman, mathematics. Finite projective geometries, 1 yr, $6100.

Morgan State College. L. I. Mishoe, mathematics. Eigenfunction series for a non-self adjoint system, 1 yr, $4600.

New York University. A. Douglis. Hadamard's conjecture and related problems in the theory of wave propagation, 1 yr, $7500.

Oberlin College. J. D. Baum, mathematics. Topological dynamics, 1 yr, $3600.

University of California, Berkeley. J. Neyman, mathematics. The distribution of galaxies, 2 yr, $21,800.

Oklahoma Agricultural and Mechanical College. E. W. Titt, mathematics. A new approach to partial differential equations, 2 yr, $26,000.

University of California. A. Tarski, mathematics. Theory of models, 2 yr, $19,800.

University of Georgia. M. K. Fort, Jr., mathematics. Functional equations, 1 yr, $6500.

University of Illinois. A. H. Taub, mathematics. Analytic program for digital computer, 1 yr, $14,500.

University of Southern California. H. Busemann, mathematics. Geometry with nonsymmetric distances, 2 yr, $6600.

University of Tennessee. W. Givens, mathematics. Continuous geometry, 18 mo, $13,300.

University of Michigan. M. O. Reade, mathematics. Subharmonic, harmonic, and analytic functions, 2 yr, $12,600.

University of Michigan. E. E. Moise, mathematics. Topology of manifolds, 30 mo, $13,600.

University of Minnesota. H. Yamabe, mathematics. Structure of manifolds, 2 yr, $11,100.

University of Wisconsin. R. H. Bing, mathematics. Imbedding sets in manifolds, 2 yr, $17,000.

California Institute of Technology. R. B. Corey, chemistry. X-ray diffraction studies of crystalline proteins, 3 yr, $36,000.

California Institute of Technology. L. Pauling, chemistry. Configurations and polypeptide chains in proteins, 3 yr, $30,000.

Columbia University. S. Lieberman, obstetrics and gynecology. Steroid hormone biosynthesis by perfused human placenta, 3 yr, $25,000.

University of Illinois. J. Larner, chemistry. Carbohydrate absorption; synthesis and degradation of polysaccharides, 3 yr, $16,000.

Pennsylvania State University. R. Pepinsky, physics. Nucleic and desoxynucleic acid fragments, 2 yr, $20,000.

Amherst College. T. Soller, physics. Metals and paramagnetic salts below 0.1°K, 2 yr, $19,500.

University of California, Berkeley. D. S. Saxon, physics. Theoretical nuclear and atomic physics, 2 yr, $21,100.

Indiana University. R. W. Thompson, physics. Construction of a double cloud chamber for research on fundamental particles, 2 yr, $78,900.

Midwestern Universities Research Association, University of Illinois. D. W. Kerst, University of Illinois. High-energy accelerator problems, 9 mo, $69,400.

University of Pennsylvania. K. R. Atkins, physics. Superfluidity of liquid helium, 2 yr, $22,200.

University of Pennsylvania. W. E. Stephens, physics. Photonuclear and transmutation processes, 2 yr, $24,700.

Principia College. S. L. Leonard, physics. Direct pair production by electrons of 200 to 500 Mev energy, 2 yr, $4200.

University of Rochester. M. P. Givens, Institute of Optics. A study of solids with soft x-rays, 2 yr, $14,200.

Stanford University. W. K. H. Panofsky, physics. Design study for high-energy magnetic spectrometers, 1 yr, $15,600.

Vanderbilt University. I. Bloch, physics. Normal modes of vibration of nuclei, 1 yr, $9300.

University of Washington. J. H. Manley, physics. Nuclear emulsion studies of pion-proton scattering, 3 yr, $9000.

Yale University. V. W. Hughes, physics. Atomic beam magnetic resonance investigations, 2 yr, $26,300.

Harvard University. E. G. Heinemann, psychology. Simultaneous contrast in human vision, 18 mo, $7600.

Harvard University. P. Teitelbaum, psychology. Effect of hypothalamic lesions on behavior, 1 yr, $5000.

Lehigh University. N. B. Gross, psychology. Neurophysiological processes in the auditory cortices, 2 yr, $9600.

Pennsylvania State University. J. F. Corso, psychology. Neural quantum theory of hearing, 1 yr, $7100.

Stanford University. C. P. Stone, psychology. Behavior of hypophysectomized rats, 2 yr, $9100.

University of Washington. M. H. Smith, Jr., psychology. Aspects of biological motivation 2 yr, $8900.

Yale University. P. D. MacLean, psychiatry. Effects of hippocampal seizures on conditioned behavior, 1 yr, $7500.

University of California, Berkeley. S. Roberts, physiological chemistry. Hypothalamic regulation of pituitary function, 3 yr, $24,000.

University of California, Berkeley. L. Machlis, botany. Metabolic pathways in the filamentous fungus, *Allomyces*, 3 yr, $15,400.

Indiana University. E. D. Weinberg, bacteriology. Mutual effects of metallic ions and antibiotics, 2 yr, $6500.

Rutgers University. A. F. Hopper, zoology. Role of thyroid gland in fish, 2 yr, $4000.

University of Texas. J. A. Scott and E. M. Macdonald, medical branch. Nature of racial or species immunity, 2 yr, $12,000.

Washington University, St. Louis. T. Rosebury, bacteriology. Interactions of microorganisms indigenous to man, 2 yr, $12,250.

University of Michigan. P. Dansereau, botany. Phytosociological studies in the Canary Islands, 1 yr, $3500.

University of South Carolina. H. W. Freeman, biology. A study of fish in the Wateree River system, 2 yr, $6300.

American Academy of Arts and Sciences. P. G. Frank. The acceptance of scientific theories, 2 yr, $22,000.

Carnegie Institution of Washington. Preliminary studies of a committee on radio astronomy, $6800.

University of Michigan. Preliminary studies of the National Astronomical Observatory Panel, $9700.

Attendance at international meetings

Second Inter-American Congress of Psychology. American Psychological Association, $2000.

Interim Council of the International Union of Biochemistry. S. Ochoa, biochemistry, New York University College of Medicine, $640.

Interim Council of the International Union of Biochemistry. E. Stotz, biochemistry, University of Rochester, School of Medicine and Dentistry.

Conferences in support of science

American Anthropological Association, Peabody Museum. Fifth international congress of anthropology and ethnology, $10,000.

American Association for the Advancement of Science. International arid lands symposium and conference, $10,000.

American Psychological Association. Conference on evolution of behavior, $8600.

California Institute of Technology. Conference on the theory of numbers, $5000.

National Academy of Sciences. International conference of marine biological laboratory directors, $4000.

University of Rochester. Fifth annual conference on high-energy nuclear physics, $4500.

Wenner-Gren Foundation for Anthropological Research, Inc. Conference on man's role in changing the face of the earth, $12,000.

University of Wisconsin. Conference on metabolic aspects of transport across cell membrances, $4600.

Education in the sciences

University of Michigan. Research and training at the University of Michigan Biological Station, 1 yr, $6900.

Scientific information exchange

Indiana University. Study on the history of psychology as a natural science, $2300.

Massachusetts Institute of Technology. V. H. Yngve. Methods of translating languages by machine, $18,700.

National Academy of Sciences. Committee on international scientific unions, Office of International Relations, $7500.

Smithsonian Institution. Biological sciences information exchange, $22,000.

Smithsonian Institution. Publication of an annotated bibliography of termites, $3600.

Scientific manpower

American Institute of Physics. Maintaining the national register of scientific and technical personnel in the field of physics, 15 mo, $14,500.

In the Laboratories

Under the Colombo Plan, India will receive $33,600 from Canada for the establishment of a **Biological Control Laboratory.** In this connection a field station will be set up at Bangalore to collect beneficial insects and other organisms in India and to import other insects and organisms with a view to utilizing them to destroy crop pests in India.

The Government of Canada, with the concurrence of the Government of India, will provide an expert who will be assisted by an Indian scientist. Other countries will be able to secure parasites and predators from the laboratory.

Beckman Instruments, Inc., Fullerton, Calif., has acquired the Specialized Instruments Corp. and Spinco Service Co., both of Belmont, Calif. These companies, to be operated as the Spinco Division of Beckman Instruments, manufacture and distribute a line of instruments for advanced research in chemistry, biophysics, and medicine.

Seven industrial companies have signed as participants in the nuclear-reactor research program at **Armour Research Foundation** of the Illinois Institute of Technology in Chicago. They are Arc Equipment Co., Bryan, Ohio; Armour and Co., Chicago; Borg-Warner Corp., Chicago; Elgin National Watch Co., Elgin, Ill.; Illinois Tool Works, Chicago; Richardson Co., Melrose Park, Ill.; and U.S. Steel Corp.

A new corporation to be known as the **General Ultrasonics Co.** has been organized in Hartford, Conn., by Alexander S. Keller, Stanley R. Rich, and Wilfred Roth. The company will handle the Rich-Roth "400" Ultrasonic Generator and associated transducers and will manufacture and market other industrial ultrasonic processing equipment.

Rich and Roth are the inventors of the Ultra-Viscoson, the ultrasonic instrument for the continuous automatic measurement of viscosity.

Established by Act of the Congress as the Government Hospital for the Insane, **St. Elizabeths Hospital** in Washington, D.C., observes its centennial this year. As part of the celebration, a meeting will be held 5–6 May featuring invited guest speakers of international reputation. Among the several "firsts" with which the hospital is credited are the use of the malaria treatment for paresis and the adoption of psychodrama as a valuable therapeutic tool. Winfred Overholser, the present superintendent, is the fifth the hospital has had in its 100-yr existence.

Recently the **Standard Oil Development Co.** changed its name to Esso Research and Engineering Co. The change was of company name only and affected neither the policies nor operation of the company.

A **sinter plant** for producing iron oxide from pyrite ashes recently began operation at the Mo and Domsjö sulfite mill at Alfredshem in northern Sweden. This is the first such plant in the world to be installed in the cellulose industry. By means of the sinter process, the waste ash, a by-product, is now converted into a cokelike substance easy to transport and useful, without further processing, in the production of iron.

Erratum: In the issue of 4 Feb. one line of the affiliation was inadvertently omitted from the heading to the article "A method of rapidly transferring a substance on paper to the origin of a chromatogram" by Garold F. Gregory. Mr. Gregory's address is Iowa State College, Ames, Iowa.

Erratum: In the news note on the President's proposed budget for the National Institutes of Health that appeared on page 155 of the 4 Feb. issue of *Science*, the first figure given, $6,399,000, is the operating budget, not the total budget.

Book Reviews

Neuere Probleme der Abstammungslehre — Die transspezifische Evolution. Bernhard Rensch. Ferdinand Enke Verlag, Stuttgart, ed. 2, 1954. xi + 436 pp. Illus. Paper DM. 47.00; cloth, DM. 49.20.

Practically every chapter and subchapter of Rensch's book on "trans-specific evolution," that is, the origin of systematic categories higher than the species, ends with a sentence that sounds approximately like this:

> In summary we can conclude that the reviewed evidence shows that the considered evolutionary event can be explained in terms of the factors responsible for intra-specific evolution (mutation, changes in population size, selection, isolation). There is no reason to assume that other autonomous internal factors have been at work.

Since these statements come from one of the world's leading zoologists who at one time was a convinced Lamarckist and later has probably contributed more than anybody else to establish the importance of geographic variations within the species in his classical work on *Rassenkreise*, the statements certainly do carry a great weight for anyone who is interested in the study of evolution.

This is the second enlarged edition; the book originally appeared in 1947. Even though the general plan has not been basically altered, several chapters have been entirely rewritten and others have been added. Trans-specific evolution is considered from two main aspects: "cladogenesis," that is, the branching of phyletic lines, and "anagenesis," that is, the origin of progressive changes, of higher more complex structures. Problems of general significance for biology, such as evolutionary rates, irreversibility of evolution, origin of new organs and of morphological correlations, orthogenesis, degeneration and extinction of phyletic branches, increase of complexity in the structure of organisms, and the like, are discussed with great originality, with the evaluation of an imposing amount of pertinent data. Chapters dealing with problems to which the author and his school have made personal contributions are particularly noteworthy. Remarkable among these is a discussion of the significance of allometric growth for evolutionary advances based on very recent investigations carried out at Rensch's laboratory at the University of Münster. Stimulating, too, are other chapters of a more speculative nature, such as that on the possibility of existence and evolution of living things on cosmic bodies other than the earth and that on the origin of the phenomenon of consciousness and cognition (*Bewusstseinerscheinungen*).

The significance of Rensch's contribution to evolutionary literature lies, I think, in the fact that it provides a thorough discussion from a modern viewpoint of problems of evolution as approached by a morphologist studying living fauna. These have previously been presented by Dobzhansky from the viewpoint of the geneticist, and therefore dealing primarily with variation at the subspecific level; by Simpson from the viewpoint of the paleontologist, and therefore dealing primarily with the major features of evolution observed along the time axis; and by Mayr from the viewpoint of the systematist, and therefore dealing primarily with speciation and the origin of taxonomic categories.

In this book we find a very learned and novel discussion, especially for the American reader, of most of the classic problems of morphology that have aroused the doubts of many biologists concerning the validity of the Darwinian theory of natural selection for explaining evolutionary events. It is indeed gratifying to see how convergent conclusions can be drawn starting from such different viewpoints and experiences. The value of genetic mechanisms in explaining morphological correlations could possibly have been presented in a more forceful way had recent work on polygenic inheritance and on correlated responses under selection been considered. As it stands, however, this book is certainly very important at the present stage of development of evolutionary studies. A translation into the English language would be very valuable.

At a time when even scientists seem to favor the intervention of mysterious, unanalyzed, and often unanalyzable, almost mystic factors to explain natural events, it is indeed refreshing to read such a clear, down to earth discussion of the major problems of biology carried out with a detached and healthy scientific attitude.

ADRIANO A. BUZZATI-TRAVERSO
Scripps Institution of Oceanography

The Mind and the Eye. A study of the biologist's standpoint. Agnes Arber. Cambridge Univ. Press, New York, 1954. 146 pp. $3.

The author of this group of essays is a well-known British botanist who, nearing the end of her active service, lays before us these studies of the basic assumptions, investigational methods, and modes of written description currently accepted among biological workers, together with her conception of the metaphysical and philosophic implications of her science. Part I deals with the nature of biological research, the choice of a problem, the mode of discovery, the interpretation of data, the validity of conclusions, and the writing of reports. Part II carries the argument to higher levels, with examination of the bases of scientific thinking, of fundamental assumptions in biological research, of the role of antitheses in the description of problems, and of the value of metaphysical and philosophic theories in interpretation. The author illuminates her argument with many

biological examples, often chosen from her own discipline. She quotes frequently from the literature of philosophy. The style is clear and precise, giving a sense of deep sincerity in a search for an understanding that will transcend appearances and find unity in the midst of diversity.

The concluding chapter, setting the theme for the whole book deals with the relationships between sensory impressions and the higher mental life. The author writes:

> Kant . . . wrote that it is essential for the achievement of abstract thought "to emancipate the mind from the despotism of the eye." To speak of "despotism" in this connection, prejudices the case from the outset; it is an unfair word, since the eye is, rather, the servant of the mind, to which it offers all its data for interpretation. . . . The activities of the sense organs, and the thinking of the brain, are all parts of an indivisible whole. . . . Whereas *Metaphysics* studies "being" as such, and *Natural Science* (of the physico-chemical type) treats of the corporeal world, *Natural Philosophy* may be so defined as to link the two; it would connote that mental activity which ceaselessly weaves connexions between the planes of intangible "essence" and tangible "existence."

The professional philosopher may find little novelty in the development of such ideas, but the biologist, usually immersed in the minutiae of observation or experiment, will do well to read this modest volume. Provisional and imperfect it surely is, as the author well knows, but it points a way toward deeper thinking about basic causes and meanings which most biologists have lost. She hopes that

> . . . its very inadequacies may stimulate others to cast an illumination, more powerful than my rushlight, upon the biologist's road to reality.

WILLIAM R. AMBERSON
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Relative Chronologies in Old World Archaeology. Robert W. Ehrich, Ed. Univ. of Chicago Press, Chicago, 1954. xii + 154 pp. Illus. Paper, $2.50.

These papers were originally presented at a joint symposium of the American Anthropological Association and the Archaeological Institute of America. Nine specialists from different regions attempted to build up a chain of chronological equations that would enable the student of comparative archeology to estimate the relative age of various cultures and to observe the contacts between them.

The assignment was an ambitious one and difficult to follow in the original, oral presentation. The carefully edited book that has resulted is an invaluable tool for the professional archeologist and an interesting demonstration of method for outsiders. Highly recommendable is Helene J. Kantor's opening paper on the situation in Egypt, cornerstone of any chronological construction, whether relative or absolute, in the ancient world. From here we wander through Palestine along a system of safe throughways to Northern Syria and Anatolia (where R. J. Braidwood and Hetty Goldman unveil new and precious footholds). We begin to feel somewhat uncertain as we turn west into the Aegean and become lost in Europe, only to be rescued by the vigorous editor.

The other road leads east, into safe and relatively well explored Mesopotamia, on via Iran into the depths of China. Here a remarkable contrast occurs. Where contacts are rather unknown, the layman will have little trouble in following the story (China). Where precise knowledge is beginning to accumulate (Iran), the account becomes highly technical and appetizing for experts only.

Attention is focused on *relative* chronology, a wise procedure. The material used is the everyday equipment of ancient man; witness the pots all over the text and cover of the book. There are moments when one would like to see *art* introduced into the story; after all we do have sculpture to tell us about Mesopotamia and North Syria. But the everyday criterions, when handled with circumspection as they are in the best of these papers, have allowed old world archeologists to resuscitate and articulate the world of early human progress.

MACHTELD J. MELLINK
Department of Classical Archaeology,
Bryn Mawr College

Biochemistry and Human Metabolism. Burnham S. Walker, William C. Boyd, and Isaac Asimov. Williams & Wilkins, Baltimore, ed. 2, 1954. xii + 904 pp. Illus. $10.

By changing the classical order of the topics of study, placing greater emphasis on proteins and amino acids, and by accenting human biochemistry rather than organic chemistry, the authors of *Biochemistry and Human Metabolism* have successfully combined the fundamentals of biochemistry with its clinical applications. The general plan of the first edition was not altered except for some minor changes, such as the inclusion of the section on acids and bases in the first chapter rather than in the appendix. The Brønsted-Lowry concept of acids and bases is concisely explained and utilized to correct the prevalent though erroneous view among some clinicians that sodium or potassium ions are bases. Chapter 14 on "Proteins and starvation" has been completely rewritten and considerably enlarged.

Although protein and enzyme chemists may be enthusiastic about the prominent place given to these subjects, the need for a 40-page chapter on "Reproduction and heredity" and a 22-page chapter on "Cancer" in a biochemistry textbook may be questioned, especially when lipids and lipid metabolism are discussed in only 16 and 20 pages, respectively.

Typographical errors are, in general, rare. It is anomalous that *amid* is used for *amide* (p. 91), while the now outdated *tryptophane* for *tryptophan* is still

retained. Formulas for tristearin and various fatty acids are given in a "long" form, such as CH_3CH_2-$CH_2CH_2CH_2CH_2CH_2CH_2CH_2CH_2CH_2CH_2CH_2$-$CH_2COOH$ for palmitic acid rather than the conventional shortened form, $CH_3(CH_2)_{14}COOH$ (pp. 119–121).

In general the book should prove to be a valuable teaching aid for instructors and a stimulating textbook for medical students. To quote from the foreword by John T. Edsall, it

> . . . has a freshness and vitality, in its general outlook and in the pattern of the presentation which give it a distinctive place among all the texts of biochemistry for medical students of which I am aware.

WALTER FRAJOLA
Department of Physiological Chemistry,
Ohio State University

The Theory of Metals. A. H. Wilson, Cambridge Univ. Press, New York, ed. 2, 1953. 346 pp. Illus. $8.50.

This edition is a new work, not only in format, but also in that it is mostly rewritten and has been considerably enlarged. This, in spite of the fact that two chapters of the former edition (the one on optical phenomena and the one on superconductivity) and also the appendix on surface phenomena, including rectification, have been eliminated. Some of the other 10 chapters follow the original outline with the ones on metallic structures and the structure of alloys now forming a special chapter each. A great deal of progress has been made in the last 10 or 15 years, particularly in the understanding, preparation, and theory of semiconductors. These are now discussed in a special chapter, but conductivity in semiconductors and thermoelectric effect, as well as magnetoresistance of semiconductors, are treated in the chapters on the formal theory of conduction and on the mechanism of conductivity. These two chapters are the core of the present work and are the most interesting and most carefully prepared. The book closes with an application of the variational principle to conduction phenomena (a method that was introduced in 1948 by Kohler and expanded by Sondheimer); although mathematically more difficult, it is more powerful. The very large amount of both experimental and theoretical material that has been accumulated in the last decade has made it necessary to limit the material discussed to a certain arbitrary selection of fields.

The British literature is discussed in some detail, but a large amount of material, particularly that of the Russian literature, such as the investigations of Pekar on effective mass, the important investigations of Gurewich on the contribution of phonons to thermoelectricity at low temperatures, and Shifrins' investigations on semiconductors, has not been discussed by Wilson.

The chapter on thermal and magnetic properties of metals contains a very careful discussion of lattice specific heat, electronic specific heat, and a comparison of the experimental data on specific heat with theory. Some of the magnetic properties, such as spin paramagnetism of free electrons and diamagnetism of free and quasi-bound electrons, as well as the rather complicated phenomena of the de Haas-alphen effect, are discussed in detail and up to date.

One may wish that in a future edition the chapter on semiconductors—for the theory of which the author has laid the foundation—would be somewhat enlarged and brought up to date. It is a surprise to read "that cuprous oxide has been given more attention than any other semiconductor." With all the past and present work on germanium it is also somewhat surprising that a numerical example chosen for germanium is one that would hardly be found in practice (10^{20} impurity centers/cm^3 and an activation energy of 0.03 ev). The author is well known for his careful and elegant mathematical deductions and considerations. The reader will find a large amount of material, particularly in the chapter on the mechanism of conductivity, that is not available in this form in other books in the field.

KARL LARK-HOROVITZ
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Abhandlungen aus der Sowjetischen Astronomie. Folge II. Gesellschaft für Deutsch-Sowjetische Freundschaft; Otto Singer, Ed. Verlag Kultur und Fortschritt, Berlin, 1951. 223 pp. Illus. DM 12.20 ($2.93).

This volume contains 13 German translations of Russian papers published in 1950. Most of these are of cosmogonical interest which is not surprising as Russian astronomers have made important contributions in this field in recent years. The publication of these volumes, as the publication of companion volumes on Russian physics, can only be heartily welcomed since the number of Western scientists capable of easily reading Russian papers in the original is still much smaller than the number of those who can read German.

The volume opens with two papers by Schain and Hase (*Uspekhi Fiz. Nauk* **43**, 3 [1950]; *Izvest. Krimskoi Astrofiz Obs.* **5**, 24 [1950]) on the occurrence of C^{13} in stellar atmospheres.

The second group of papers is by Parenago and Massewitsch (*Astr. Zhur.* **27**, 41, 137, 150, 202, 329 [1950]). Two papers deal with the mass-luminosity-radius relation. The first paper considers the empirical data and the second tries to give a theoretical interpretation of these data. The other three papers in this group deal respectively with star velocities showing the differences between high and low velocity stars (Baade's populations are, however, nowhere mentioned!), with the gravitational potential of our galaxy, and with masses of eclipsing binaries.

Then follows a paper by Gurewitsch and Lewin (*Astr. Zhur.* **27**, 273 [1950]) on the formation of

binaries. This discusses the statistics of formation and disruption of binary systems without, however, mentioning Chandrasekhar's work in this field.

Next is a polemic between Woronsow-Weljaminow and Ambarzumjan (*Astr. Zhur.* **27**, 211, 228 [1950]) on the question of whether or not hot giants occur in so-called associations—which play such an important role in recent Russian cosmogonical theories.

The longest paper of the volume is one by Ambarzumjan (*Soob. Bjurakonskoj Obs.* **6**, 3 [1951]) which summarizes his work and that of his group on fluctuations and their importance for apparent star distributions on the celestial sphere.

After an article by Hetmanzew and Ginsburg (*Zhur. Eksp. Teoret. Fiz.* **20**, 347 [1950]) on the possibility of localizing radio sources by studying the diffraction of radio waves by the moon, the volume closes with an article by Woronsow-Weljaminow (*Astr. Zhur.* **27**, 285 [1950]) on planetary nebulae.

D. ter Haar

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The Manual of Antibiotics, 1954–1955. Henry Welch, Ed. Medical Encyclopedia, New York, 1954. (Order from American Pharmaceutical Assoc., 2215 Constitution Ave., N.W., Washington). 87 pp. $2.50.

This book presents for the first time in one source a ready reference to antibiotics and their preparation. It lists the preparations, therapeutics index, trade and generic names, and the names and addresses of producers and manufacturers of all existing antibiotics and their preparations commercially produced and on markets at the time of publication.

It should prove valuable as a reference, because of the multiplicity of trade names for the same antibiotic, to members of the health profession, especially the physician, dentist, veterinarian, pharmacist, and others engaged in the use of these drugs.

The antibiotics and their preparations are alphabetically tabulated by their generic terms. The trade names given these products by each manufacturer are listed side by side with the generic equivalent. Under each of the generic terms is found the indication for each drug and preparation. The antibiotic preparations also, for ease of use, are alphabetically tabulated, both by trade and generic terms in separate indexes and in an index of all manufacturers with their addresses.

It is necessary to check only the trade-name index to identify a trade-name product. Opposite the trade name in question is the page number on which is the generic term, along with the active ingredients and indications for the preparation, in addition to all other trade names assigned to the products.

The author states that periodic revision of this manual is planned to keep up to date the ever-increasing list of antibiotics and their preparations.

Hurd M. Jones, Jr.

School of Pharmacy, Texas Southern University

Formation des Continents et Progression de la Vie. H. Termier and G. Termier. Masson, Paris, 1954. 135 pp. Illus. + plates. Paper, Fr. 750.

Orogenesis and tectonics, stratigraphy and paleontology are carefully used to introduce the reader to the initial appearance and subsequent expansion of life on the earth. Little credence is put in phantom continents, or those that have had only a legendary existence, and the authors work toward an over-all synthesis that precludes acceptance of the continental drift theories of Wegener and Argand. The Termiers make a distinct contribution by giving present-day examples of phases of the geologic process, illustrated, for instance, by the photo of a group of starving hippopotamuses wallowing in a diminutive mudhole left by the drying up of Lake Rukwa in Tanganyika in 1950. These huge mammals were unable to escape catastrophe by migrating in time to a more humid area.

Even the spectacular geologic phenomena obey regular laws, but a disconformity was experienced in the evolutionary process with the appearance of man who is capable of thought processes. The lack of specialization of his hands and the possession of a brain gave him superiority over all other animals. The authors regard as significant the fact that up to about 100,000 years ago man lived only in the hot and subtropical zones. During the first three glacial epochs man migrated equatorward with the other fauna. By the time of the fourth period of glaciation he could clothe and warm himself and was able to adjust to climatic extremes. Modern man has proved to be a powerful *geologic* agent, in such activities as mining coal, damming streams, reclaiming land from the desert and from the sea, and so on.

This succinct, well-written work is recommended to the general reader.

Raymond E. Crist

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Pigment Cell Growth. Proc. of the Third Conference on the Biology of Normal and Atypical Pigment Cell Growth. Myron Gordon, Ed. Academic Press, New York, 1953. 365 pp. Illus. + plates. $7.

It may be questioned whether or not the study of pigment cell growth is developing rapidly enough to justify publication of comprehensive reports every few years. Nevertheless, for anyone who wants to become familiar with current work and find a guide to that of the past, this volume forms an adequate introduction.

The papers consist for the most part of more or less extensive reviews of recent investigations in pigmentation. To a biologist, the coverage will seem to be narrow—that is, to be weighted on the side of human and mammalian pigment cells. Two papers are concerned with structure of melanins and melanin synthesis, three with lower vertebrates (fish, frog, axolotl), two with the chicken, five with the mouse, and ten with man. The emphasis is also heavy on disease:

there are eight papers on melanomas, two on carcinomas, and one on *acanthosis nigricans*, compared with five on biochemistry of pigment, two on embryology, two on genetics, and one on the relation of endocrine glands to pigmentation in the fowl. The invertebrate animals are not treated.

As is usual in publications of this kind, the papers vary in length and quality. The longest, as well as the most comprehensive, paper is that on the structure of melanins by H. S. Mason, a valuable review of the chemistry of melanin formation. The book is well printed on glossy paper and extensively illustrated with graphs and photographs. The reproduction of the halftones is first rate.

One gratifying outcome of the conference was an agreement on the terminology to be used for pigment cells. The definitions adopted for melanoblast, melanocyte, macrophage, and melanophore were also approved by the Subcommittee on Oncology, Division of Medical Sciences of the National Research Council and, hence, may be expected to have a more uniform use in the future.

GRAHAM DUSHANE
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Explaining the Atom. Selig Hecht. Rev. by Eugene Rabinowitch. Viking, New York, 1954. xviii + 237 pp. Illus. $3.75.

It is a well-known fact that the distinguished scientist, when writing a popular book on his special field, underrates the difficulty of the ideas he is familiar with and so produces a book for his colleagues but not for the general public. The outstanding popular book on the atomic bomb has been written not by a nuclear physicist but by a great expert in another field of science. In 1946 Selig Hecht, professor of biophysics, wrote *Explaining the Atom*, the book that gave to many thousands an understanding of atomic structure and nuclear energy. The author's incentive was as much scientific interest as a sense of responsibility of the citizen who sees a new world coming and wants to propagate an understanding of the driving forces.

After the untimely death of the author, Eugene Rabinowitch brought the book up-to-date by many minor changes. The vivid style of Selig Hecht, who told the story of discoveries rather than described detail, is fully preserved. Hecht's book ends with the sections "The atomic bomb is built," and "The secret is out."

Rabinowitch, known to the public as editor of and contributor to the *Bulletin of the Atomic Scientists*, continues the story to the level of 1954. He describes the later development of the fission bomb and, more fascinating, the new "superbomb." Here he discusses Bethe's carbon cycle and the fusion reactions between hydrogen isotopes and lithium leading to thermonuclear reactions. They allow the construction of the famous bomb whose size is not subject to limitations (except the carrying capacity of a plane) and whose price is presumably much lower than that of a large fission bomb. A separate chapter is devoted to "Atomic power."

The chapters contributed by Rabinowitch emphasize the idea of Selig Hecht: to inform the citizen of the indispensable background of physics as well as to point out the relation of nuclear energy to the industrial development and domestic and foreign policy. The scope of fission is evident from the recent estimate that "the world resources of commercially utilizable fissionable material are fifty times greater than the world resources of commercially utilizable fossil fuels." This estimate, however, seems to include all uranium and thorium irrespective of the efficiency of the breeding process which leads to the fissionable isotopes.

Rabinowitch closes with an optimistic prospect:

> We live on the continuous but finite surface of a sphere of which any part can be reached from any other part in a few hours. It is obsolete to suppose that such a surface can be artificially maintained in a fractional state of national groups . . . the sooner all peoples join in some law-abiding extranational order, the better for us who hope for civilization.

The second edition of Selig Hecht's book is as excellent as the first edition for the instruction of the citizen who wants a sound foundation for his judgment on domestic and international policies as affected by the new energy.

O. OLDENBERG
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Linear Transient Analysis. vol. I, *Lumped-Parameter Two-Terminal Networks.* Ernst Weber. Wiley, New York; Chapman & Hall, London, 1954. xiv + 348 pp. Illus. $7.50.

Linear Transient Analysis is a textbook containing material appropriate for a basic graduate course in transient analysis. It is also of value to practicing engineers who will find four methods of solving linear transient problems gathered together in one volume. The book is unique in this respect. In presenting the classical solution of network response, the Heaviside-Jeffreys' operational calculus, the Laplace transformations, and the Fourier transform, Weber has laid bare the mystery of transients. To those devotees of the Laplace transform who would shun the classical or Heaviside methods, he answers:

> Fundamental knowledge, real understanding of any subject matter, must be independent of the form in which it is presented or in which we had our first introduction to it. The greater the variety of possible expressions for the same basic relationship, the clearer will be the concept recreated in the mind of the searching individual.

The chapters are laid out in a well-integrated order. The first chapter deals with concepts of circuits and networks that might ordinarily be overlooked in undergraduate work but are the essence of advanced circuit analysis. Classical solutions of network re-

sponse follow in the second chapter where solutions of first- or second-order systems are presented for a number of source functions. The third chapter is a 27-page digression into the fascinating area of analogues and duals. Lagrange's equations of motion and electromechanical systems are treated briefly in this chapter. Following the chapter on Heaviside-Jeffreys' operational calculus, there is a treatment of the Laplace transform method. Here Weber relies rather heavily on physical justification for mathematical operations, but he is careful to point this out. The indicial response function is given importance as the key to the solution of linear transient problems involving arbitrary driving functions. The last chapter is concerned with the spectrum concept and takes up Fourier series and Fourier integrals. The treatment of what might be called nonideal periodic wave forms is a unique feature of this chapter.

There is a generous appendix of six parts: "Notation, symbols, and glossary"; "Electromagnetic fields and energy relations"; "Determining roots of polynomials"; "Matrices and determinants"; "Functions of a complex variable"; and "General bibliography." The bibliography is excellent and the footnoting of the volume is unusually liberal. Weber's prose reads well and appears to be quite adequate for its purpose.

L. Daniel Runkle

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Connective Tissues. Trans. of the Fourth Conference, Feb. 18–20, 1953. Charles Ragan, Ed. Josiah Macy, Jr., Fdn., New York, 1953. 197 pp. Illus. $3.75.

The informal conversational style of these conferences is again followed in this meeting. The chapter titles indicate in only a very general way the content. They are "General areas of agreement reached in this conference group"; "Isolation and characterization of mammalian striated myofibrils"; "The effect of vitamin A on organ cultures of skeletal and other tissues," and "Outline of problems to be solved in the study of connective tissues."

The first chapter is extremely interesting in that it brings out the almost abysmal problem of word meaning and the difficulties in communication that necessarily follow. The discussants formulated an outline related to connective tissues, but they had only "relative unanimity" and not real agreement on the concepts involved. In my opinion a statement by one of the discussants very nearly summarizes this chapter:

> I think we are quibbling. We are reaching the point where we are merely using words and I think none of us knows what he is talking about.

The next two chapters are concerned with some extremely interesting observations on myofibrils and the effect of vitamin A on certain tissues. The pertinence of these subjects toward increasing our understanding of the scope and function of connective tissues seems rather remote.

The last chapter is devoted to a discussion of the problems of the structure and function of connective tissue. Reference is made to the difficulties encountered in trying to correlate the observations of the histochemist and biochemist on the localization and content of connective tissue substances. One of the most interesting problems advanced was that of whether the parenchymatous structures depend on the surrounding connective tissue stroma for their functional capacity. This concept was discussed by Gersh and Catchpole in 1949 and has since been beautifully elaborated upon by them and others.

This little volume is interesting but is not a valuable addition to this field.

John A. Arcadi

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Grundlagen und Praxis chemischer Tumorbehandlung. Zweites Freiburger Symposion an der Medizinischen Universitäts-Klinik. J. Pirwitz. Springer, Berlin, 1954. 289 pp. Illus. Paper, DM. 45.

Cancerization of cells is an irreversible process consisting of damages that gradually involve a great many self-duplicating cytoplasmatic particles of macromolecular structure (Druckrey) and result in preinvasive intraepithelial carcinomas of ½- to 20-yr duration (Schubert). Cancer is not a problem of heredity (K. H. Bauer). Cancer in adults might be the response to agents that acted in childhood. [It is apparently unknown in Germany that this was already recognized and proved in 1939 (S. Peller, *Cancer in Man,* International Univs. Press, New York, 1952, pp. 307–310)].

Most anticancer agents are cytostatica. They attack growing cells during the late interphase, when deoxyribonucleic acid is being duplicated (Marquardt). True mitotic poisons are rare. Within the limits in which rapidly multiplying normal tissues are not severely damaged, the available agents do not kill cancer cells but weaken them enough to give the broken-down defense mechanism a chance (Lettré, Domagk). Mutations of the treated cancer cells cause the diminishing effectiveness of therapy (Burchenal). Since the chemical deviations of the cancer cell from the normal cell are too small, development of cancer-cell-specific poisons is unlikely. Cytostatica have much in common with x-rays (radiomimetica, Boyland). Surgeons should utilize chemotherapy against cells disseminated during operation, and so forth (Kraus). So far no human cancer was cured by chemotherapy (Heilmeyer, Pirwitz). Cures of prostatic cancer might be achieved by modification of chemotherapy—by intravenous administration of large amounts of inactive stilbestrol diphosphate that is split and activated in all prostatic cancer cells (Raabe, Rockstroh, Brock).

This symposium created an opportunity for an inspiring and fruitful exchange of ideas on matters essential to every cancer investigator.

Sigismund Peller

New York, N. Y.

New Books

Theory of Functions of a Complex Variable. vol. II. C. Caratheodory. Trans. by F. Steinhardt. Chelsea, New York, 1954. (Published by Birkäuser, Basel, 1950). 220 pp. $4.50.

Building Materials: Their elasticity and inelasticity. M. Reiner, Ed. vol. VII of *Deformation and Flow.* Monogr. on the rheological behaviour of natural and synthetic products. J. M. Burgers, J. J. Hermans, and G. W. Scott Blair, Eds. North-Holland, Amsterdam; Interscience, New York, 1954. 560 pp. $12.25.

A Check List of the E. DeGolyer Collection in the History of Science and Technology as of August 1, 1954. Compiled by Arthur McAnally and Duane H. D. Roller. Univ. of Oklahoma Press, Norman, ed. 3, 1954. 127 pp. Paper, $3.

Advances in Virus Research. vol. II. Kenneth M. Smith and Max A. Lauffer, Eds. Academic Press, New York 10, 1954. 313 pp. $7.

Advances in Carbohydrate Chemistry. vol. 9. Melville L. Wolfrom, Ed. Academic Press, New York, 1954. 426 pp. $10.50.

Aspects of Deep Sea Biology. N. B. Marshall. Philosophical Library, New York, 1954. 380 pp. $10.

An Autumn Gleaning. Occasional lectures and addresses. Henry Hallett Dale. Interscience, New York; Pergamon, London, 1954. 225 pp. $4.25.

The Chemistry of Living Cells. Helen R. Downes. Harper, New York, 1955. 549 pp. $7.50.

Chemistry and Physiology of Mitochondria and Microsomes. Protoplasmatologia, vol. III, A4. Olov Lindberg and Lars Ernster. Springer, Vienna, 1954. 136 pp. $8.10.

Clinical versus Statistical Prediction. A theoretical analysis and a review of the evidence. Paul E. Meehl. Univ. of Minnesota Press, Minneapolis, 1954. 149 pp. $3.

Conserving Natural Resources: Principles and practice in a democracy. Shirley W. Allen. McGraw-Hill, New York–London, 1955. 347 pp. $5.50.

K* (****Krebiozen—Key to Cancer?***) Herbert Bailey. Hermitage House, New York, 1955. 312 pp. $3.50.

Cryptogamic Botany. vol. II, *Bryophytes and Peteridophytes.* Gilbert M. Smith. McGraw-Hill, New York–London, 1955. 399 pp. $8.

Ephraim's Inorganic Chemistry. P. C. L. Thorne and E. R. Roberts. Interscience, New York and Oliver and Boyd, Edinburgh–London, ed. 6, 1954. 956 pp. $6.25.

How to Know the Minerals and Rocks. Richard M. Pearl. McGraw-Hill, New York–London, 1955. 192 pp. $3.50.

Inventories of Apparatus and Materials for Teaching Science. vol. III, *Technical Colleges*; pt. 4, *Electrical Engineering.* UNESCO, Paris, 1954 (Distrib. by Columbia Univ. Press, New York 27). 147 pp. Paper, $2.75.

Active Transport and Secretion. Symposia of the Society for Experimental Biology, No. VIII. R. Brown and J. F. Danielli, Eds. Academic Press, New York, 1954. 516 pp. $8.

Analysis of Deformation. vol. II, *Experiment and Applied Theory.* Keith Swainger. Chapman & Hall, London, 1954. 365 pp. 70s.

Analysis of Development. Benjamin H. Willier, Paul A. Weiss, and Victor Hamburger, Eds. Saunders, Philadelphia–London, 1955. 735 pp.

Animal Agents and Vectors of Human Disease. Ernest Carroll Faust. Lea & Febiger, Philadelphia, 1955. 660 pp. $9.75.

The Gyroscope Applied. K. I. T. Richardson. Philosophical Library, New York, 1954. 384 pp. $15.

Miscellaneous Publications

(Inquiries concerning these publications should be addressed, not to *Science*, but to the publisher or agency sponsoring the publication.)

Ordnance. Patent Abstract Ser., No. 6. Prepared by Govt. Patents Bd. Dept. of Commerce and Small Business Administration, Washington 25, 1954 (Order No. 111469 from Office of Tech. Services, Dept. of Commerce, Washington 25). 58 pp. $2.

Comprehensive Dental Care in a Group Practice. A study of service and time requirements. Public Health Service Publ. No. 395. Dept. of Health, Education, and Welfare, Washington 25, 1954 (Order from Supt. of Documents, GPO, Washington 25). 48 pp. 25¢.

Contributions to the Theory of Partial Differential Equations. Annals of Mathematics Studies, No. 33. L. Bers, S. Bochner, and F. John, Eds. Princeton Univ. Press, Princeton, N.J., 1955. 257 pp. $4.

The New Atomic Energy Law—What It Means to Industry. Proc. of a meeting, 27–28 Sept. 1954. Oliver Townsend, Ed. Atomic Industrial Forum, New York 16, 1954. 181 pp. $5.

Archeological Investigations in the Oahe Dam Area, South Dakota, 1950–51. River Basin Surveys Papers, No. 7. Bur. of American Ethnology Bull. 158. Donald J. Lehmer. Smithsonian Institution, Washington, 1954 (Order from Supt. of Documents, GPO, Washington 25). 190 pp. $1.50.

The Rockefeller Foundation, Annual Report for 1953. The Foundation, New York, 1954. 482 pp.

Achievements in Federal Reorganization. The contributions of the first and new Hoover commissions. Ray Harvey, Louis W. Koenig, and Albert Somit. Citizens Committee for the Hoover Report, New York 17, 1955. 30 pp. 50¢.

Railroad Photos Old and New. Association of American Railroads, Washington 6. 15 pp. Gratis.

Abridged Scientific Publications from the Kodak Research Laboratories. vol. 32, 1950. Eastman Kodak Co., Rochester, N.Y., 1954. 320 pp.

Soil Density and Stability. Highway Research Bd., Bull. 93. Natl. Acad. of Sciences–Natl. Research Council, Washington 25, 1954. 64 pp. 90¢.

Evolution and Taxonomy of the Sarcophaginae. (***Diptera, Sarcophagidae***). Illinois Biological Monogr., vol. 23, Nos. 3–4. Selwyn S. Roback. Univ. of Illinois Press, Urbana, 1954. 181 pp.

Contribution of Atomic Energy to Medicine. Hearings before the Subcommittee on Research and Development of the Joint Committee on Atomic Energy. 83rd Congress. Second session, 2–4 June 1954. Joint Committee on Atomic Energy, Washington 25, 1954. 194 pp.

Report of the Conference on Astronomical Research in Small Academic Departments. Held at Sproul Observatory, Swarthmore College, 19–21 April 1954. Natl. Science Foundation, Washington 25, 1954. 12 pp.

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Technical Papers

Resistance of Saccharomyces to High Concentrations of Lithium Chloride

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Yeasts frequently occur in mediums containing high concentrations of salts, that is, in food brines (*1*) or in sea water (*2*). Mrak and Bonar noted that the ability of several species of *Debaryomyces* to grow in high salt concentrations was reduced when the cultures were grown for a time on wort agar without additional salt. Takada and Nagai (*3*) investigated the plasmolytic behavior and the content of amino acids of *Saccharomyces ellipsoideus* adapted to high concentrations of NaCl. Growth and variability of this yeast on NaCl medium was studied by Yanagishima (*3*).

In experiments on the genetics of resistance of *Saccharomyces* (*4*) to relatively high concentrations of a variety of salts, several strains were obtained that can grow in a synthetic medium containing lithium chloride in concentrations up to 1.0*N* and do not lose their resistance after transfer in normal medium. The resistant strains were obtained by exposing unadapted stock cultures to various concentrations of the salt, which was added to DIFCO yeast nitrogen base medium, with 1-percent glucose as the carbon source. When liquid medium was used, 5000 to 20,000 cells were inoculated in 5 ml of the medium plus salt concentrations of 0.5*N* to 1.0*N*. Growth was observed in some tubes after 8 to 10 days; the cultures that grew were transferred to fresh salt medium every second or third day. The strains thus obtained remained resistant after 12 transfers in normal medium. Resistant mutants were also obtained on solid medium containing LiCl in this same range of concentrations. Ten to 20 days after plating, a comparison with the control plates (without added LiCl) revealed that only a fraction of the cells had produced colonies on the salt medium. Some of the largest colonies were selected, and these remained resistant after several transfers in normal medium.

Twenty-seven independent haploid and diploid strains were thus obtained, all of which can grow on 0.5*N* and some on 1.0*N* LiCl medium. In order to analyze the genetic basis of the resistant strains, more than 60 crosses of various combinations were made. The diploids resulting from crosses between haploid LiCl-resistant strains and nonresistant strains grew, in all cases, on LiCl medium. Therefore, resistance to LiCl is dominant to nonresistance. Evidence of the genetic basis of resistance to LiCl was obtained by dissecting asci from several crosses and testing the spore cultures. Sixteen crosses were made and four nonallelic dominant genes (L_1, L_2, L_3, and L_4) have thus far been identified (*5*). Two of these genes (L_1 and L_2) were already present in two strains at the beginning of the experiments, whereas the other two (L_3 and L_4) arose after exposure of the strains to medium containing LiCl (as described earlier). It is not yet known whether LiCl is mutagenic in its effect or simply serves as a selective agent for the accumulation of mutants of spontaneous origin.

The response of four haploid strains carrying each of the dominant genes for LiCl resistance to increasing concentrations of LiCl is given in Table 1. For comparison, the response of a nonresistant haploid strain of the recessive genotype $l_1\ l_2\ l_3\ l_4$ is also shown. It will be noted that strains carrying gene L_1 exhibit fair growth on 0.3*N* LiCl medium within 6 days of incubation. Strains carrying gene L_2 exhibit good growth within 4 days on 0.5*N* LiCl medium, and strains with L_3 or L_4 need only 2 days to show good growth on the same medium. On concentrations of 0.75 to 1.0*N* LiCl medium, only strains containing the genes L_2, L_3, or L_4 will grow. The last produces the most vigorous growth.

A spectrophotometric analysis of resistant cultures grown on LiCl medium has shown a considerable decrease of the potassium content of the cells as compared with the controls of the same cultures grown

Table 1. Response of haploid strains carrying different genes for LiCl resistance to increasing concentrations of LiCl.

Genotypes for resistance to LiCl	Normality of LiCl concentration in the medium*							
	0	0.1	0.2	0.3	0.4	0.5	0.75	1.0
$l_1\ l_2\ l_3\ l_4$	+ (2)†	+ (4)	± (6)	–	–	–	–	–
$L_1\ l_2\ l_3\ l_4$	+ (2)	+ (3)	± (4)	± (6)	–	–	–	–
$l_1\ L_2\ l_3\ l_4$	+ (2)	+ (2)	+ (2)	+ (2)	+ (3)	+ (4)	± (5)	± (7)
$l_1\ l_2\ L_3\ l_4$	+ (2)	+ (2)	+ (2)	+ (2)	+ (2)	+ (2)	+ (5)	± (5)
$l_1\ l_2\ l_3\ L_4$	+ (2)	+ (2)	+ (2)	+ (2)	+ (2)	+ (2)	+ (2)	+ (2)

* Growth on solid mediums of different concentrations of LiCl is roughly compared with the following symbols: (i) + is good growth, equal or nearly equal to control culture growing on LiCl-deficient medium; and (ii) ± is fair growth, less efficient than growth of control.

† The numbers in parentheses give the average number of days necessary for the indicated state of growth.

on normal medium. Only 20 to 30 percent of the original potassium content could be found when the cultures were grown in 1.0*N* LiCl medium or were exposed to a 1.0*N* LiCl solution for 24 to 48 hr. The sodium content of the cells did not show a significant change in any case. In cultures grown on LiCl medium, lithium could be demonstrated only in the cells if the cultures were not washed more than twice. Nonresistant strains, grown on normal medium, were also exposed to LiCl solutions for 24 to 48 hr. They showed the same decrease in the potassium content as the resistant strains grown on the salt medium. The sodium content of the nonresistant strains exposed to LiCl solutions was also unchanged in comparison with control cultures.

The decrease of the potassium content after treatment with LiCl is of special interest since it is known that the K^+ ion plays an important role during fermentation in the yeast cell (*6–8*). It has also been pointed out that Li^+ interferes with the carbohydrate metabolism in yeast, sea urchin larvae (*9, 10*), and bacteria (*11*). An antagonistic effect of Li^+ and K^+ in yeast with regard to glucose fermentation was reported by Lindahl (*10*). Whether there is a direct exchange in the yeast cell of K^+ by Li^+ or whether Li^+ acts as a glycolytic inhibitor like iodoacetic acid or sodium fluoride which causes the loss of K^+ (*7*) remains to be determined. The fact that no Li^+ could be found in the yeast cells after several washes does not support the first possibility.

On the basis of these facts, it may be stated that the effect of LiCl on the decrease of the potassium content is the same in nonresistant and resistant strains. The ability of the latter to grow on LiCl medium is under gene control. If potassium is normally essential to the cell (*6–8*), the mutation enables the cell to grow in the presence of LiCl with a considerably decreased amount of potassium. Therefore, it can be assumed that a general difference exists between mutant and normal cells in regard to their requirement for a minimal amount of potassium.

This assumption could be supported by the results of fermentation tests in a glucose medium lacking LiCl and deficient in potassium (*12*). Several LiCl-resistant and nonresistant strains were inoculated into test tubes containing 8 ml of the medium, part of which filled a small inverted test tube (Durham tube). After 1 wk of incubation it appeared that only the LiCl-resistant strains had filled the inverted tubes with gas, whereas the nonresistant strains had produced no gas or only a small amount of it. In addition, it should be mentioned that strains containing the genes L_2, L_3, or L_4 filled the inverted tubes with gas in a shorter period of time than strains with the gene L_1. Further experiments to determine the significance of potassium for normal and mutant strains are in progress (*13*).

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12. The medium used was a modification of DIFCO yeast nitrogen base medium (with glucose as carbon source) in which the potassium salts were replaced by sodium salts. The possibility remains that traces of K^+ were still present as impurities of other compounds. For the present purpose, however, it was considered as sufficient if the K^+ content was largely decreased in comparison to the standard medium.

13. This investigation was supported in part by funds from grant E-328, National Institutes of Health, Public Health Service, and the Biological and Medical Fund of the State of Washington. I am grateful to R. B. Walker for an introduction to the methods of operating the flame spectrophotometer.

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Hemocyanin and Radioactive Copper

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In view of the recent revival of interest in the binding of copper to the hemocyanins (*1–4*), we wish to report some observations made several years ago in these laboratories (*5*), that emphasize the extremely low degree of dissociability of the copper-protein bond. During the preliminary phase of a series of investigations in which radioactive copper has been used to study the exchangeability of copper in the copper enzymes, ascorbic acid oxidase (*5–7*), and tyrosinase (*8*), we had occasion to carry out an exploratory study of copper exchange in the hemocyanin of *Busycon canaliculatum*.

Filtered whole *Busycon* serum containing 0.22 to 0.26 percent copper based on the dry weight of the serum was employed. The procedures including the methods of radioactivity assay for the hemocyanin-radiocopper exchange experiments were similar to those previously published describing the exchange studies on ascorbic acid oxidase (*6, 9*). Unbuffered columns of Amberlite IR-100 (Na^+), 25 by 0.8 cm, were used for separating from the protein solution the unbound radiocopper remaining in solution after the hemocyanin had been exposed to the radiocopper for the desired length of time. All experiments were carried out at *p*H 6.5 to 7.0, approximating the natural *p*H of *Busycon* serum.

Table 1. Removal of extraneous ionic copper but not native copper from hemocyanin by Amberlite IR-100 (Na^+).

Expt.	Prepn.* (1 ml)	Cu content (μg)	Amt. of Cu^{++} added (μg)	Time of contact with added Cu^{++} (hr)	Cu content of effluent (μg)
1	1H*	1.0	0	0	1.0
2	1H	10.0	10.0	20	9.7
3	1H	10.0	10.0	20	9.8

* For description of this hemocyanin preparation see first footnote of Table 2. In experiment 1 the hemocyanin was diluted (1 : 10). Experiments 2 and 3 were duplicates.

It was found that hemocyanin can be passed slowly through an Amberlite column with no loss in its native copper. This result itself affirms the high stability of the copper-protein linkage, a property long recognized by virtue of the nondialyzability of the copper. It was also shown that when an amount of ionic copper equal to the copper content of the hemocyanin was added to the serum and the mixture passed through the column, the extraneous ionic copper was removed and the effluent solution had a copper content equal to that of the original hemocyanin (Table 1). All copper determinations were performed by the method of Warburg and Krebs (*10*).

The nearly quantitative recovery of nonionic copper has been taken as evidence for the quantitative recovery of hemocyanin protein. Since no measurements of physiological properties, such as oxygen capacity, were performed on the hemocyanin solutions before or after their passage through the resin columns, only qualitative evidence that the protein was not damaged is available. Thus it was observed that all of the effluents still possessed the characteristic blue color of oxyhemocyanin, and had the ability to be decolorized reversibly by sodium hydrosulfite, a property shown only by native hemocyanin (*11*).

When radioactive cupric ions were added to a solution of oxyhemocyanin for periods of time up to 16 hr, it was found that no significant radioactivity was incorporated into the hemocyanin (Experiments *A*, Table 2). It may be concluded, therefore, that under the conditions of the experiment no exchange occurred between radiocupric ions and the oxyhemocyanin copper.

The possibility of an exchange reaction between hemocyanin copper and radiocopper ions while the hemocyanin was undergoing reversible oxygenation–deoxygenation was investigated. It is well known that fresh blue solutions of oxyhemocyanin, when placed under a vacuum, readily lose oxygen and decolorize rapidly. When the decolorized solutions are shaken in air, they become blue again. An amount of radioactive cupric ions equal to the copper content of the hemocyanin was added to freshly prepared hemocyanin solutions. Evacuation of the mixture with a water pump vacuum for 5 min resulted in decolorization; shaking with air for 1 min restored the blue color. This process was repeated 5 times, after which the mixtures were passed through an Amberlite column. The effluents were analyzed for copper content and radioactivity. The results, shown in Table 2 (Experiments *B*), indicate that no significant exchange occurred while the hemocyanin was undergoing reversible oxygenation–deoxygenation.

From the work of Kubowitz (12) it appears that the copper in hemocyanin is in the cuprous form. It has also been suggested that dissociation of the cuprous ion from unoxygenated hemocyanin may proceed more easily than from the oxyhemocyanin (*4*). It is clear, however, that such dissociation did not occur to any significant extent under the conditions of our experiments. If it had occurred, the following equilibrium system would have been established, and radioactivity would have been incorporated into the hemocyanin via the cuprous ion, that is

$$\text{Hemocyanin-Cu}^+ \rightleftarrows \text{Cu}^+ + \text{apohemocyanin}$$
$$\text{Cu}^+ + {}^*\text{Cu}^{++} \rightleftarrows {}^*\text{Cu}^+ + \text{Cu}^{++}$$
$$\text{Apohemocyanin} + {}^*\text{Cu}^+ \rightleftarrows \text{hemocyanin-}{}^*\text{Cu}^+$$

Table 2. Hemocyanin and Cu^{64}. Experiments type *A* involved oxyhemocyanin exposed to radioactive cupric ions for two different periods of contact before passage through an Amberlite IR-100 (Na^+) column to remove the extraneous cupric ion. The *B* experiments are duplicates and involved hemocyanin undergoing reversible oxygenation-deoxygenation in the presence of radioactive cupric ions prior to passage through the column.

Experiment		Prepn.* (1 ml)	Cu content (μg)	Amt. Cu^{64} added (μg)	Time of contact with Cu^{64} (hr)	Cu in effluent (μg)	Radioactivity (counts/min)		Exchange (percent)
Type	No.						Effluent	Comp. stand.†	
A	1	1H	10.0	10.0	1	not detd.	4 ± 3	818 ± 12	0
A	2	1H	10.0	10.0	16	9.8	2 ± 2	324 ± 6	0
B	1	2H	12.1	12.1	0.5	11.7	0	3850 ± 27	0
B	2	2H	12.1	12.1	.5	not detd.	0	3850 ± 27	0

* Preparation 1*H* was obtained from a stock solution of hemocyanin 2 yr old. After dialyzing for 2 wk against copper-free water, the copper content was found to be 0.25 percent. Preparation 2*H* was a fresh solution of hemocyanin (3 days old); it contained 0.22 percent copper.

† The method of preparing the comparison standard has been previously described (6).

It is known that if both cuprous and cupric ions are present in solution, the radioactivity cannot be localized at either ion, regardless of which ion is originally tagged (*13*).

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Thermodynamic Analysis of the Intracellular Osmotic Gradient Hypothesis of Active Water Transport

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The problem of transporting water from a solution of high osmolarity to one of low osmolarity has engaged the attention of biologic investigators for many years. The intracellular gradient hypothesis, and the mechanism for the maintenance of the gradient, formulated in detail by Franck and Mayer (*1*), seemed to us, on superficial examination, to be a reasonable working hypothesis. An alternative mechanism for the maintenance of osmotic gradients involving the flow of electric current through ion-selective circuits was considered (*2*). Detailed examination of the implications of such a system brought to light certain fundamental objections. It became apparent that such objections applied with equal validity to any intracellular osmotic gradient scheme.

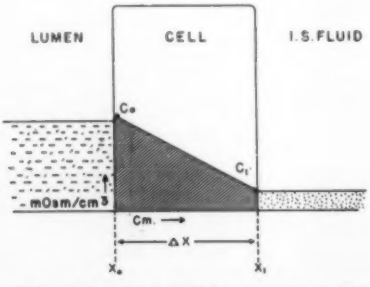

Fig. 1. Scheme of an intracellular osmotic gradient. The osmotic activity C is plotted as a function of cell thickness ΔX.

To illustrate the analysis, consider the application of the osmotic gradient hypothesis to the process of formation of a hypertonic urine. When (Fig. 1) the intracellular osmotic activity at the lumen side of the cell, C_0, is slightly higher than that of the lumen fluid, and the intracellular osmotic activity at the interstitial fluid side of the cell, C_1, is equal to that of the interstitial fluid, water could be transported from the lumen to the interstitial fluid. Consider the analysis applied to those cells concerned with active water transport, under conditions where the gradient is maintained but in which no water is being transported, that is, C_0 is equal to the osmotic activity of the lumen fluid and C_1 is equal to that of the interstitial fluid. The results of analyses applied to both a flat-sheet and a tubular arrangement of water-transporting cells were of a similar order of magnitude. For simplicity the flat-sheet arrangement of cells was chosen for presentation. The number of solute particles diffusing from the lumen side of the cell to the interstitial side must be equal to the number of solutes transported in the opposite direction by the mechanism maintaining the gradient. Under such steady-state conditions, the number of solute particles transported can be estimated from the integrated form of Fick's equation,

$$Q_0 = -D(C_1 - C_0)/\Delta X, \qquad (1)$$

where Q_0 is equal to the number of osmols per square centimeter per second diffusing from X_0 to X_1, D is the diffusion constant, and ΔX is equal to the cell thickness. During the production of a hypertonic urine, reasonable values for the parameters in Eq. 1 are $C_0 = 1.5$ mOsm/cm³, $C_1 = 0.3$ mOsm/cm³, $\Delta X = 2 \times 10^{-3}$ cm, and $D = 2.0 \times 10^{-5}$ cm²/sec. Upon substitution of these values in Eq. 1 and conversion of the units, it is found that Q_0 is equal to 4.3×10^{-2} osmol/cm² hr.

The rate of change of free energy for the diffusion process may be evaluated by using the well-known formula

$$\delta(\Delta F)/\delta t = -Q_0 RT \ln(C_0/C_1), \qquad (2)$$

where ΔF is the change in free energy, t the time, R the gas constant, and T the absolute temperature. Since diffusion is an irreversible process, the free-energy decrease of the diffusion process cannot be funneled back into the transporting mechanism. Substitution in Eq. 2 for a temperature of 37°C gives a value of 4.3×10^{-2} kcal/cm² hr, which represents the minimum rate of expenditure of free energy for the uphill transport of the solutes.

To calculate the rate of change of free energy per unit volume, a specific gravity of 1.0 for cells was assumed. The minimum rate of expenditure of free energy that is required to maintain the gradient is then found to be 21,000 kcal/kg hr, which is approximately 1000 times the maximal rate for living cells.

The discrepancy becomes all the more apparent in considering the fact that a reasonable value for the efficiency of most biological mechanisms is about 20 to 30 percent.

This analysis renders untenable the Franck-Mayer hypothesis for the maintenance of the osmotic gradient. However, it does not rigorously exclude an osmotic gradient hypothesis for the transport of water if (i) the water-transporting cells have a metabolic rate more than 1000 times greater than the maximal value reported for mammalian tissues, (ii) a gradient-maintaining mechanism can be found that would be capable of funneling energy from a high fraction of the total cellular mass to a few milligrams of water-transporting mass, (iii) there were cells of inordinate thickness ($\Delta X > 6$ cm), or (iv) there existed in cells small solute particles of high osmotic activity with a much smaller diffusion constant than that recorded for the largest protein molecule. The analysis will be presented in detail elsewhere (*3*).

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A Study of Leucine Biosynthesis in *Torulopsis utilis*

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As part of a study of biosynthetic mechanisms, the formation of the branched chain amino acids, valine, isoleucine, and leucine in *Torulopsis utilis* is under investigation using isotope tracer methodology. Data are presented in this paper (*1*) that indicate that the leucine carbon chain consists of an acetyl group attached to the isobutyryl moiety of valine. In previous studies (*2*), results of experiments were reported in which *T. utilis* was grown on glucose as the principal carbon source in the presence of variously C^{14}-labeled acetates and lactates. Valines and isoleucines were isolated from the yeast cells, they were degraded by chemical procedures, and the individual carbon atoms were assayed for radioactivity. Based on these findings, mechanisms for the biosynthesis of these amino acids have been proposed (*2*).

Using a degradation procedure similar to that employed for valine and isoleucine, C^{14}-distribution patterns were obtained for the leucines isolated from the same experiments; these are given in Table 1. Acetate carboxyl carbon was exclusively and abundantly present in the carboxyl carbon of leucine, and the acetate methyl carbon appeared overwhelmingly in the leucine α carbon. Lactate carbon 1 was incorporated to a negligible extent, hence, degradation was not conducted. Large amounts of lactate carbons 2 and 3 also appeared in the respective carboxyl and α carbons of leucine, a result anticipated on the basis of the ready conversion of lactate carbons 2 and 3 to acetate. However, lactate carbon 2 was also incorporated readily and equally in leucine carbons 3 and 4, and lactate carbon 3 also appeared in leucine carbons 5,5′. The similarity in the distribution of all three lactate carbons in leucine carbons 3 to 5,5′ to that observed previously in carbons 2 to 4,4′ of valine (*2*), shown at the right of the table, leaves little doubt of the common origin of the isobutyryl moieties of both amino acids. Gilvarg and Bloch (*3*) and Ehrensvard *et al.* (*4*) also found that acetate carboxyl and methyl carbons were incorporated into leucine biosynthesized by yeast. Adelberg (*5*) reported learning, in a private communication from Ehrensvaard, that yeast grown on acetate as the sole carbon source yielded valine and leucine with the same isotope distribution in their isobutyryl moieties. Abelson, (*6*) recently showed that pyruvate, α-ketoisovalerate and L-valine all lowered the specific activities of leucines synthesized by *Escherichia coli* from uniformly labeled glucose. He suggested that these substances are intermediates of leucine biosynthesis, and further suggested that α-ketoisovalerate combines with acetate to yield the

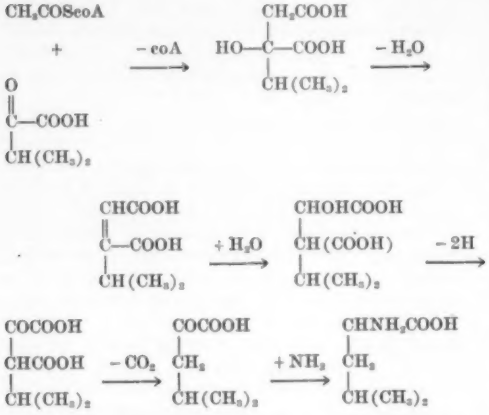
Table 1. Pattern of C^{14} distribution in leucine and valine. Values are based on standard dosage of 100 μc administered.

Substrate	Specific activity of leucine	Percentage of total activity in leucine carbon					Specific activity of valine	Percentage of total activity in valine carbon			
		5,5′ $(CH_3)_2$	4 CH	3 CH_2	2 $CHNH_2$	1 COOH		4,4′ $(CH_3)_2$	3 CH	2 $CHNH_2$	1 COOH
Acetate-1-C^{14}	14,040	0	0	0	1	99					
Acetate-2-C^{14}	21,380	3	1	2	89	2					
Lactate-1-C^{14}	156*						12,080	0	0	1	99
Lactate-2-C^{14}	25,630	2	32	33	1	31	20,670	1	47	49	3
Lactate-3-C^{14}	21,540	59	1	2	37	1	14,730	91	4	4	1

* This leucine sample was not degraded.

keto analog of leucine. The present results confirm and extend this postulation.

By analogy with the condensation of oxalacetate with acetate to yield citric acid, the following detailed mechanism is proposed for the synthesis of the leucine carbon chain.



α-Ketoisovalerate is presumed to condense with the methyl carbon of acetyl coA to yield α-hydroxy-α-isopropylsuccinic acid. By the same series of reactions undergone by citric acid to yield α-ketoglutaric acid, this hydroxy acid would be converted to α-keto-γ-methylvaleric acid, which, by transamination with glutamic acid (*7*), would yield leucine. The possible participation of these hypothetical intermediates in leucine biosynthesis is now under study (*8*). A similar reaction sequence was suggested by us to account for the synthesis of α-aminoadipic acid in connection with the biosynthesis of lysine (*9*).

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* Postdoctoral fellow of the National Institutes of Health, U.S. Public Health Service.

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22 November 1954.

Segregation of Sex Factors in a Diploid Line of *Ustilago zeae* Induced by Alpha Radiation

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During investigations of the effect of naturally radioactive heavy metals on microorganisms, a phenomenon that appears to be somatic segregation and recombination has been induced repeatedly in solopathogenic, diploid lines of *Ustilago zeae*, the common corn smut fungus, by alpha radiation. This report (*1*) on the solopathogenic line 410qq compares the segregation and recombination of the factors for sexual compatibility that occurred during meiosis in chlamydospore germination with that which was induced by the irradiation of vegetative cells.

U. zeae is normally heterothallic, and galls and chlamydospores (zygotes) are produced when corn is inoculated with pairs of compatible haploid lines (*2, 3*). Solopathogens were described by Christensen (*4, 5*) and Eddins (*6*) as monosporidial lines that by themselves produced galls and chlamydospores in inoculated corn. The cultural, sexual, and pathogenic segregants obtained during germination of the chlamydospores produced by these lines (*4, 5*) are identical to those recovered from chlamydospores of a cross of haploid lines. Since cells of solopathogens are uninucleate (*5*), they are considered to be diploid, although cytological proof of the presence of $2n$ chromosomes has not been obtained.

The solopathogenic character of monosporidial lines of *U. zeae* is not always stable. Some of the solopathogenic lines characterized by Christensen (*5*) and Stakman *et al.* (*7*) were found to be avirulent in later tests. Chilton (*8*) isolated a variant from a solopathogen that was compatible with certain haploid lines but nonpathogenic when inoculated alone in corn. The solopathogenic line 410qq used in this study was isolated in 1943 by Stakman *et al.* (unpublished) from a cross of lines 10A4 × 17D4, and the character of solopathogenicity has remained unchanged in stock cultures since that time. However, variants that were compatible with 10A4 but not with 17D4 were isolated by Gattani (*9*) from line 410qq grown on mediums containing uranyl nitrate. During the current investigation, 349 monosporidial isolates from suspensions of cells of line 410qq that were not exposed to alpha radiation were tested as controls, and all were solopathogenic.

Vegetative sporidia of the diploid line 410qq that were exposed to alpha radiation were harvested from young monosporidial cultures grown in potato-dextrose-broth (PDB) by shake culture. In the original trials, washed sporidia were exposed to alpha radiation by suspension in a solution of 1 μc/ml of polonium-210 after the method of Rowell *et al.* (*10*). For later exposures, however, the alpha radiation emitted from a 10-mc source of polonium-210 plated

Table 1. Comparison of the segregation and recombination in sex factors resulting from meiosis in germinating chlamydospores and from exposure of vegetative sporidia of a diploid line (410qq) to alpha radiation.

Sex genotype	Mating reaction of nonsolopathogenic isolates with tester lines				No. of isolates for each genotype				
					From germinating chlamydospores		From treated sporidia of 410qq		
	17D4 (a^2b^4)	822Cb (a^2b^1)	NyCe (a^1b^4)	10A4 (a^1b^1)	10A4 × 17D4	410qq	I	II	III
a^1b^1	+*	–	–	–	9	6	3	7	10
a^1b^4	–	+	–	–	1	10	3	3	4
a^2b^1	–	–	+	–	2	8	0	11	5
a^2b^4	–	–	–	+	1	6	0	0	2
$a^1a^2b^1$	+	–	+	–	12	1	3	17	6
$a^1a^2b^4$	–	+	–	+	2	1	3	15	15
$a^1a^2b^1b^4$	No mating reaction†				26	1	75	197	72

* The symbol + indicates combinations of lines that are sexually compatible, that is, produce galls and chlamydospores in inoculated corn. The symbol – indicates no reaction.

† Solopathogenic lines that by themselves produce galls and chlamydospores in inoculated corn.

on a 1-in. brass square and coated with a thin layer of gold and plastic lacquer was used to irradiate a dried, monocellular layer of sporidia on the surface of 2-percent water agar. Isolates from the survivors on dilution plates were tested for solopathogenicity and sexual compatibility by inoculating seedling corn by the partial-vacuum method of Rowell and DeVay (*11*).

The sex alleles of monosporidial lines from a cross can be determined by matings in corn seedlings with lines representing each of the four haploid combinations of the parental sex factors. Very detailed studies previously reported in a symposium paper (*12*) have established that sexual compatibility in *U. zeae* is governed by genes at two loci, one of which has only two known alleles (a^1 and a^2), while the other has multiple alleles (b^1, b^2, b^3, . . . b^n). Mated haploid lines produce galls and chlamydospores in corn only if different alleles of *a* and *b* are brought together (see mating reactions in Table 1). In addition to the haploid lines, there are amphisexual lines (*12*) that have the alleles $a^1a^2b^1$ or $a^1a^2b^4$, and these lines are compatible with any haploid line having a different *b* allele. The four tester lines used to identify the mating types of isolates in this study were 10A4 (a^1b^1), 822Cb (a^2b^1), NyCe (a^1b^4), and 17D4 (a^2b^4) in which the complement of sex alleles had been established by inheritance studies (*12*).

The number of monosporidial isolates for each sex genotype obtained by meiosis from germinating chlamydospores of the cross 10A4 × 17D4 ($a^1b^1 \times a^2b^4$) and the solopathogenic progeny of this cross, 410qq ($a^1a^2b^1b^4$), are listed in Table 1. The last three columns of the table list the number of isolates of the same genotypes found in tests of survivors from three separate irradiation trials with vegetative sporidia from line 410qq. Only cultural "mutants" were tested in trial I. Trial II was a test of a random sample of the survivors after exposure to alpha radiation, and it was found that most avirulent lines were isolated from initially slow-growing colonies with distorted cell shapes (Fig. 1). The isolates in trial III were all made from dilution plates of broth subcultures of such characteristically abnormal colonies. Thus, the changes in sex factors induced by alpha radiation in the vegetative sporidia of line 410qq were similar to those obtained during segregation and recombination from chlamydospores.

The abnormal cells were studied to determine if the observed changes in sex factors were induced immediately by the alpha radiation or were produced during subsequent growth of these cells. Ten entire young colonies of abnormal-cell survivors of irradiation were transferred to PDB. Suitable control cultures were made from colonies of untreated cells. The isolates with abnormal cells produced approximately 1000 times less growth in broth than did the control isolates. After 3 days' growth by shake culture, the resulting cell populations were sampled on dilution plates. A majority of the colonies developing from the lines with abnormal cells resembled the initial abnormal colony; in the minority were colonies of various cultural types and with the normal cell shape and growth rates of the untreated 410qq. The same results

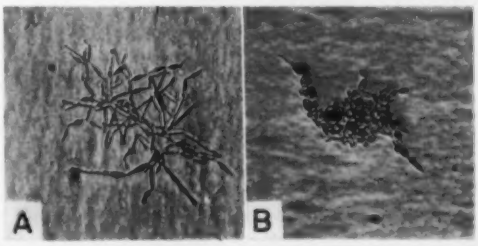

Fig. 1. Young colonies of *Ustilago zeae* developing from single sporidia of the diploid line 410qq. (*A*) Typical sporidial colony produced in 24 hr by a single, nonirradiated cell. (*B*) Colony of distorted cells produced in 72 hr by a single abnormal cell obtained after the line was exposed to alpha radiation.

were obtained when this process was repeated through each of nine successive cultural generations with subtransfers made to PDB from colonies with abnormal cells.

Representatives of the many variants with normal cell characters were isolated from the dilution plates for each of the 10 abnormal cell lines and tested for sex type by inoculating corn seedlings. As many as six different sex genotypes were found among the descendants from each original abnormal cell. However, no mutations were found in the genes for sexual compatibility. Thus, it is apparent that exposure to alpha radiation induced an unstable condition in vegetative cells of this diploid line that resulted in the segregation and recombination of the factors for sexual compatibility during multiplication of the affected cell.

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Number and Size of Radial Resin Ducts in Slash Pine

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Rate of flow of oleoresin from freshly made wounds on slash pine (*Pinus elliottii* Engelm.) is determined in part by the number and size of radial resin ducts. In this study, the relationships of these two anatomical factors with age of tree and width of growth ring were determined as part of a study on inheritance of factors that determine oleoresin yield.

Microtome sections were prepared at five-ring intervals from the vascular cambium to the pith of 10 parent trees. Wood samples, approximately ½ by 1½ in., were removed from the selected trees at breast height by boring two holes to the pith, one above the other, and sawing out the connecting wood. The samples were separated at every fifth ring, and tangential sections, 25 to 30μ thick, were cut with a sliding microtome. These were stained with haematoxylin and safranin that sharply differentiated the resin ducts and accompanying cells (*1*).

A complete count was made of the horizontal resin ducts within each section with the aid of a mechanical stage. The ducts were counted in parallel strips, using the diameter of the field of vision as the width of each strip. Counts were made under 100× magnification. The area of each section was determined from measurements with a graduated mechanical stage. The average area for each measured sample was 3.03 cm². Radial resin canals in tangential section appear elliptical; hence, the major and minor axes were measured to include thickness of the epithelial cells (Fig. 1). Ten ducts per section were measured with an ocular micrometer under a magnification of 440×.

Results of the relationships between number and size of ducts and age of ring, and between number and size of ducts and average ring width, are presented in Fig. 2. Inspection of the data showed the following relationships.

1) The number of radial ducts formed per unit area was highest during the early age (rings near the pith) and decreased rapidly until about the 20th year, after which it leveled off. In the wood close to the pith, most of the ducts are found in those vascular rays initiated contiguously with medullary rays. As the diameter of the tree increases, there is a decrease in the number of these rays per unit area of tangential surface. This decrease in number of resin ducts is partially offset by formation of additional vascular rays containing resin ducts.

2) A similar relationship existed when number and size of resin ducts were plotted against average width of ring. The gradual decrease in ring width with age contributed to the stabilization in the number of ducts.

3) Average size of resin ducts decreased linearly with age of ring. Since the size of tracheids is reflected by age of ring, this effect is probably associated with resin-duct size.

With the measurements taken it was not possible

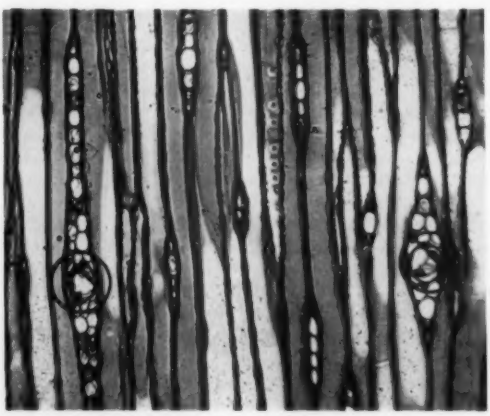

Fig. 1. Photomicrograph of two radial resin ducts on a tangential surface. The resin ducts (within circles) are contained in fusiform rays.

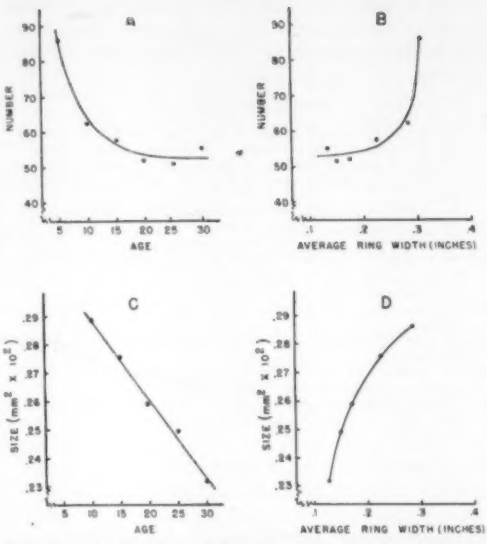

Fig. 2. Relationships of number and size of the radial resin ducts to age of the ring ($A + C$) and average ring width ($B + D$). The number of ducts per square centimeter is given.

to determine definitely whether the effect of age or the effect of ring width was more important. In the 10 trees studied, average ring width decreased with age of tree. By sampling a larger number of older trees it should be possible to find trees with an irregular growth pattern, and thereby evaluate the respective importance of age and width of ring. In a study on the gradient of wood density in trees that had wider growth rings toward the outside, Turnbull (*2*) was able to demonstrate that the density of wood formed in a particular year is not determined by the growth rate, but is proportionate to a function of age.

The relationship of number and size of resin ducts to age and width of ring exhibits a pattern similar to that of wood density and strength characteristics. The volume of resin ducts probably accounts in part for this pattern by directly influencing density and strength of the wood. Their influence is probably greatest in the first five rings where they are most abundant. On the average for the 10 trees studied, the number of horizontal resin ducts at ring 5 was 100 percent greater than at ring 25. The slight increase in the number of ducts at ring 30 is probably a result of traumatic resin ducts formed as a result of wounding for naval stores.

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23 November 1954.

Pheasant-Turkey Hybrids

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Hybrids from pheasants (*Phasianus colchicus*) and the domestic fowl (*Gallus* sp.) have been known for a long time (*1*) and have recently been more fully investigated (*2*). All hybrids out of these two species are apparently sterile. Artificial insemination (*3*), which overcomes some of the isolating mechanisms between species (*4*), has been used (*5, 6*) to obtain hybrids from the domestic fowl and turkey (*Meleagris gallopavo*), but only one hybrid hatched (*6*). Two centuries ago, Edwards (*7*) described, with an illustration, a hybrid "supposed to be bred between a turkey and a pheasant." The pheasant-turkey cross, however, has been neglected, although Sokolow *et al.* (*8*) recently reported that the chromosomes of the turkey and pheasant are more alike than those of the latter and the domestic fowl.

During an investigation of crosses between gallinaceous species made by artificial insemination in 1952, ring-neck pheasant hens were mated with bronze turkey males. At least 10 fertile eggs were obtained; six survived 2 wk or longer, and two were alive after the 24th day of incubation but did not hatch.

In 1953 reciprocal matings were made on a more extensive scale. The results are summarized in Table 1. No similar difference in fertility between the reciprocal crosses was observed in 1954 (*9*). The difference in fertility in 1953 may therefore reflect variations in intervals between inseminations, amount of semen used, and variations in the technique of insemination rather than more fundamental differences between the reciprocal crosses. Early embryo mortality of the hybrids was high, but most of the live 10-day-old embryos survived to 24 days.

The hybrids from pheasant eggs hatched after about 26 days' incubation; those from turkey eggs required 27 to 28 days. The average incubation period for pheasants is 24 days, for turkeys, 28 days. Time of hatch usually varies considerably but the hybrids in pheasant eggs clearly required a longer incubation period than pheasants.

Of the embryos that survived to 10 days of age in the 1953 hatches, the percentage hatched varied from 0 to 50 percent. In 1954 about 50 percent of the eggs

Table 1. Number of eggs set and hatched, 1953.

Type of cross	Set	Fertile	Embryo survival		Hatched
			10 days	24 days	
Pheasant ♀♀ × turkey ♂♂	381	100	44	43	13
Turkey ♀♀ × pheasant ♂♂	231	100	50	39	11

from pheasant hens and turkey males were fertile, and of these 23 percent hatched. This indicates that, under favorable conditions, better results than those shown in Table 1 may be expected.

Early posthatching mortality in 1953 was high for hybrids out of pheasant female-turkey male crosses, with only one survivor; but posthatching mortality was low for hybrids out of the reciprocal cross, with five survivors. Differences in survival were apparently due to managerial factors. The hybrids were raised with mixed groups of young birds of several species and their hybrids. Under such conditions the small turkey-pheasant hybrids from pheasant eggs fared badly at first, while the larger hybrids from turkey eggs did better. Much of the later mortality was caused by severe pendulous crop, but several of the survivors were saved by surgical removal of the crop. Provision of adequate shade or prompt surgery would undoubtedly have saved most of the birds that died after they were a few weeks old.

The mature hybrids are intermediate in weight between the turkey and the pheasant. Their head furnishings resemble those of the pheasant; the skin on the sides of the head around the eye is partly free from feathers but otherwise the head and neck are feathered (Fig. 1). When not fully grown, the feathers on the head and neck of some hybrids have been observed to resemble those illustrated by Edwards (*7*). The tail feathers are intermediate in length. The plumage color of the hybrids from this particular cross (ring-neck pheasant and Bronze turkey) is dark brown shading to black, except on the wings, which are lighter.

Neither eggs nor semen were obtained from the year-old hybrids. Mating activity was not observed, although there was some brightening of the skin on the head of the males. Thus, these intergeneric hybrids, like those from the pheasant and domestic fowl, are apparently sterile. The survival of these pheasant-turkey hybrids has, however, been superior to that reported for domestic fowl-turkey hybrids (*5, 6*), and is about equal to that of pheasant-domestic fowl hybrids.

Fig. 1. Head of a 6-mo-old hybrid out of a Bronze turkey female and a ring-neck pheasant male. Except for some variation in plumage color and area of featherless skin around the eye, it is typical of both sexes from this and the reciprocal cross.

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9. Among those who have helped with this cross, we are particularly indebted to John P. Hillerman and Norris E. Wilson.

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Leukemogenic Effects of Ionizing Radiation on Atomic Bomb Survivors in Hiroshima City

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Following the explosions of the atomic bombs in Japan, a great increase in leukemia was reported among survivors (*1*). However, statistical confirmation of the high incidence of leukemia was not possible because of the unusual distribution of survivors by age and sex. During 1953, additional cases of leukemia appeared among the survivors. Using these new cases, an analysis was carried out that established statistical evidence of the leukemogenic effects of atomic irradiation on man (*2*). The 98,000 survivors in Hiroshima City, and the 50 verified cases of leukemia occurring among them, were distributed according to distance from the hypocenter. In addition, the survivors were further classified according to the degree of irradiation. Thus, individuals with a history of epilation, oropharyngeal lesions, or purpura were classified as heavily irradiated (severe radiation complaints); those without such complaints were considered lightly irradiated (no complaints).

Statistical tests carried out on the data appearing in Table 1 indicate the following. (i) A highly significant difference in incidence exists between survivors with serious radiation complaints and those with no complaints. Within each distance group, the incidence of leukemia is higher among the survivors with serious complaints. (ii) There is a significant difference in incidence among the distance groups. For each complaint group, the incidence of leukemia is highest among survivors exposed closest to the hypo-

Table 1. Incidence of leukemia in the Hiroshima survivors related to distance from the hypocenter and the presence of severe radiation complaints.

Distance from hypocenter	Population*			Cases of leukemia			Incidence		
	SRC†	NRC‡	Total	SRC	NRC	Total	SRC	NRC	Total
0– 999	750	450	1,200	14	1	15	1: 53	1: 450	1: 80
1,000–1,499	2,250	8,250	10,500	15	9	24	1: 150	1: 917	1: 438
1,500–1,999	1,750	16,950	18,700	3	2	5	1: 583	1: 8,475	1: 3,740
2,000–2,499	950	16,250	17,200	1	1	2	1: 950	1: 16,250	1: 8,600
2,500 and over	850	49,650	50,500	0	4	4		1: 12,412	1: 12,625
Total	6,550	91,550	98,100	33	17	50	1: 198	1: 5,385	1: 1,962

* Population estimated and rounded off to the nearest 50 persons. These population figures were based on the Commission's 1949 radiation census and the Japanese national census (1950). Numbers of survivors with severe radiation complaints were estimated from observations made by the Commission's genetics department on 19,675 Hiroshima survivors of childbearing age (*3*).

† SRC: severe radiation complaints (heavily irradiated).

‡ NRC: no radiation complaints (lightly irradiated).

center. (iii) No interaction of distance and radiation complaints is evident. That is to say, the difference in incidence between the complaint groups is not dependent on the distance from the hypocenter. (iv) A linear relationship appears to exist between the logarithm of the distance and the logarithm of the incidence of leukemia. This relationship is demonstrated by a downward slope that is significantly different from zero. (v) There is no reason to believe that a difference exists between the individual regression coefficients for the two complaint groups. Thus, the rate of decrease in incidence with an increase in distance is apparently the same for the two groups.

Further examination of the data indicates that (i) the incidence of leukemia is "high" at distances close to the hypocenter, regardless of the presence or absence of severe radiation complaints; (ii) the incidence of leukemia approaches the normal expected incidence at distances of 2500 m or more from the hypocenter.

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* On leave of absence from Tufts Medical School and Boston City Hospital.

† Present address: University of North Carolina, Chapel Hill.

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22 December 1954.

Communications

Pollen Profiles, Radiocarbon Dating, and Geologic Chronology of the Lake Michigan Basin

Wave action of the high-water stage of Lake Michigan during the past 3 yr cut deeply into flanking dunes along the eastern shore of the lake and did millions of dollars of damage to summer homes, bathing beaches, and scenic highways along the shore. Below South Haven, Mich., on the property of the summer camp operated by the Michigan Congregational Christian Conference, the waves uncovered a 30-in. layer of compacted peat perched on a 5-ft layer of sand and covered by a 25-ft dune.

The peat apparently accumulated in an interdunal pond whose bottom was sealed by an iron precipitation. The vertical cut of the peat permitted easy sampling at 1-in. intervals for pollen analysis. A trench dug to a depth of 75 in. into the sand underlying the compacted peat terminated in a basal layer of blue silt capped by a layer of wood. Samples were taken of the blue silt and of all narrow organic streaks that appeared in the 75-in. layer of sand. All samples contained sufficient pollen for a very satisfactory pollen analysis and determination of the forest history.

The 30 in. of peat record four major forest changes and an equal number of intermediate changes. The blue silt and the 75-in. layer of sand were deposited during a very prominent spruce-fir period (up to 94-percent spruce-fir pollen). The compacted peat began during the late spruce-fir period when pine showed aggressive participation in the forest cover. The succession of forests indicated in the pollen profile of the 30 in. of peat was spruce-fir-Jack pine to Jack pine-spruce-fir to Jack pine to Jack-white (red) pine (spruce and fir almost extinct) to white-red pine, to pine-oak-chestnut to oak-pine to oak-pine-hemlock-broadleaved forest.

The closing layers of the peat mark the beginning

of the climatic optimum when dune activity buried the pond and compressed the peat. Radiocarbon dating (Table 1) by the University of Michigan laboratory gave the following time placement of forest changes: beginning of the pond near the close of the spruce-fir period, 8000 yr ago; the pine period, 6000 yr ago; the oak-pine period, 5000 yr ago; close of the pond at the beginning of the climatic optimum and initiation of dune building, 4000 yr ago. From the profile of a 32-ft deep bog, 15 mi inland from Lake Michigan, we know that during the past 4000 yr the climatic optimum continued to develop, as shown by increase of oak, hickory, and pine, and that in most recent times climatic deterioration occurred during which change pine has increased in abundance.

The geologic chronology of the Lake Michigan basin that can be fitted into this forest and climatic history is as follows. The blue silt underlying the 75-in. layer of sand is correlated with the Glenwood stage of Lake Chicago in the Lake Michigan basin. A radiocarbon date of 11,000 yr for the woody layer immediately overlying the blue Glenwood silts indicates that the deposition of the silts was followed by a withdrawal of the strandline, thus allowing the blue silts to be exposed to subaerial conditions, during which time the woody layer accumulated. The woody layer is correlative in time with the Bowmanville low water phase.

Following this a return to the later stages of Lake Chicago—the Calumet II stage of Bretz (*1*), and Tolleston stage—and Lake Algonquin is indicated by continuous deposition of sand containing lenses of organic material, all of which accumulated in relatively shallow water behind a barrier beach. These lacustrine sands eventually emerged from beneath the surface of the water as a result of the opening of the North Bay outlet and the fall of Lake Algonquin to the extreme low-water phase which Hough (*2*) named Lake Chippewa. During this marked decline in water level (350 ft below modern Lake Michigan) a stream connected Lake Chippewa in the Lake Michigan basin with Lake Stanley in the Huron basin by way of the present Strait of Mackinac. It was also during this rapidly falling water level that dune activity ceased in the vicinity of South Haven because of the withdrawal of the strand line that constituted the source of the dune sand. The dunes became stabilized with forest cover and the 30 in. of peat accumulated in an interdunal depression. Radiocarbon dates from the top and bottom of the peat layer reveal that the bog existed for about 4000 yr.

Uplift of the North Bay outlet caused the water level in the Michigan-Huron basins to rise to the level of Lake Nipissing. Return of the strandline to this level reinitiated dune action at South Haven; this resulted in burial of the peat and the building of a 25-ft dune above it. Since that time the shore has been eroded until the high water of 1952 revealed the buried peat layer.

The age determinations from the University of Michigan Radiocarbon Laboratory (Table 1) are the basis for an absolute chronology of the Lake Michigan basin. Sample numbers have the prefix M to distinguish them from the sample numbers of other radio-

Table 1. Radiocarbon dates.

No.	Sample	Age (yr)
M-288a	Wood from the top of the basal blue silt at the South Haven site. Dates the Bowmanville low water phase in the Lake Michigan basin.	11200 ± 600
M-288	Peat from the lowest 2 in. of the 30-in. peat layer at the South Haven site. This dates the time when the waters of Lake Algonquin had already begun to drop to the Lake Chippewa level. The date is thus a minimum for Lake Algonquin and the time that the North Bay outlet became ice-free. Judging from the pollen at this level in the peat, the waning phase of the spruce-fir period in the South Haven latitude is also coincident with this event.	[8350 ± 500 7500 ± 500] Average, 7925 ± 400
M-289	Peat from the South Haven exposure, 7 in. above the base of the 30-in. peat layer. Stratigraphically above M-288. A post-Algonquin–pre-Chippewa date. According to the pollen profile of the South Haven peat, this sample also dates the pine period.	6330 ± 400
M-290	Wood from the central part of the South Haven 30-in. peat layer. Interpreted as a date for the minimum level of Lake Chippewa in the Lake Michigan basin. According to pollen at this level of the peat, this date marks the oak-pine period at the South Haven latitude.	[5000 ± 400 5185 ± 400] Average, 5090 ± 300
M-291	The upper 2 in. of peat in the South Haven exposure. Marks the time just before dune activity was renewed as a result of the return of the water from the Chippewa low water phase to the Nipissing stage. This date thus just precedes the Nipissing maximum and, according to the pollen at this level, just precedes the Xerothermic (oak-pine-hemlock-broadleaved forest in the South Haven latitude). By inference, then, the Nipissing stage and the Xerothermic period were coincident.	[4000 ± 160 4000 ± 350] Average, 4000 ± 350

carbon laboratories. The samples are numbered in stratigraphic order, oldest (M-288a) to youngest (M-291).

Several conclusions may be safely drawn from this study: (i) The time lapse between the Algonquin and Nipissing stages in the Lake Michigan basin was 4000 yr (M-288 to M-291), with Lake Algonquin ending about 8000 yr ago (M-288) when the North Bay outlet became ice free. (ii) Lake Chippewa intervened between the Algonquin and Nipissing stages in the Lake Michigan basin about 5000 yr ago (M-290) and was coincident with the oak-pine period in southwest Michigan. (iii) The Xerothermic period reached a maximum some time after 4000 yr ago and is coincident in time with the Nipissing stage in the Lake Michigan basin.

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4 January 1955.

Unusual Reagent

The authors of an article in the *Journal of Biological Chemistry* [**211**, 168 (1954)] acknowledge a gift of a sample of "standard human brain inhibitor." Presumably this is the basis for man's chronic difficulties in meeting the test. Who will discover the antidote?

DAVID E. GREEN
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6 January 1955.

Bacitracin

Experiments in this laboratory on the stability of bacitracin have uncovered the interesting fact that bacitracin will form an irreversible gel with certain chemicals.

Two grams of bacitracin dissolved in 5 ml of water, alcohol, or acetone, or mixtures thereof, will form a gel in the presence of 200 mg or less of anethole, anisole, cinnamic aldehyde, and isosafrol. The time for gel formation varies with the chemical and concentration. No gel is formed in the presence of menthol, isopropyl benzene, benzocaine, ascorbic acid, oleic acid, eugenol, isoeugenol or safrol. In the presence of isopropenyl benzene (insoluble in the system) or morpholine, no gel is formed. Using morpholine as a solvent a gel is formed with the bacitracin and isopropenyl benzene. The presence of a conjugated ring system seemed necessary to gel formation except for the anomalous behavior of isoeugenol.

The gelling phenomenon occurred incidental to certain of our pharmaceutical development work and was expanded to the extent described. It is reported here as isolated behavior of bacitracin in the hope that it may be correlated with more direct investigations on the composition of the antibiotic.

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6 January 1955.

The Individual in Chemical Research

It is a fairly general belief that the individual researcher in chemistry has been supplanted in modern times by the research team. Numerical data to support this generalization apparently have not been collected.

Information on this point can be obtained by counting the number of papers in chemical journals that are written by a single author rather than by groups. There are some obvious flaws in this procedure as a measure of individual scientific endeavor, but the results are, at least, interesting. The percentage of papers written by only one author in the *Journal of the American Chemical Society* (Table 1) has indeed decreased since 1918, indicating a probable decline in individual research.

A further indication that few lone-wolf chemists exist in modern research is offered by the fact that not more than 15 authors in any one year (for the years given in Table 1) published more than two papers without coauthors.

Since the *Journal of the American Chemical Society* is sometimes accused of being primarily for organic chemists (in 1940, for example, five organic chemists were senior authors of about 10 percent of the published papers; since then the percentage from these individuals has declined), a check was also made of the *Journal of Chemical Physics* where organic chemists probably would not be represented. In both 1940 and 1950 this journal had approximately 40

Table 1. Individual papers in the *Journal of the American Chemical Society.*

Year	Total papers*	No. by single author	Percentage
1918	220	100	45
1920	302	131	43
1928	487	169	35
1930	838	244	29
1938	937	183	20
1940	1084	180	17
1948	1557	256	16
1950	2022	282	14

* Taken from reports by the editor published in the *Journal* itself or in *Chemical and Engineering News*; it does not include book reviews.

percent of its contributions written by a single author. Of course, most of the writers for this journal probably classify themselves as physicists rather than as chemists.

The real frontier for the individual in science, however, would appear to be in mathematics. In 1950, 98 percent of the papers in the *American Mathematical Monthly* were written by individuals.

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17 December 1954.

Soda Fe-Mn Pegmatite Phosphates

The crystallography and thermal relations of alluaudite, dickinsonite, and fillowite, the last three minerals for which data are given in Table 1, are here discussed; no new data have been obtained for natrophilite (Table 1) or the little-known varulites. Each of these five soda-phosphate minerals consists of a solid solution Fe-Mn series. The first two minerals listed, chondrodite and fayalite, are silicates; they are included to make clear the orientation used, which is that of chondrodite with $c < a$. The olivines (represented in Table 1 by fayalite) are isotypous with the pegmatite phosphate triphylite. The latter has a crystal structure similar to but not identical with that of natrophilite (*1*). The new unit-cell data for the last three minerals of the table were obtained from Chanteloube alluaudite and Branchville dickinsonite and fillowite. For Black Hills ferrodickinsonite or arrojadite (*2*), $a = 24.78$, $b = 10.05$, and $c = 16.51$ A.

In a series of thermal experiments it was found that the Black Hills ferrodickinsonite, when heated for a day in air, alters to alluaudite between 485 and 555°C. On the other hand, Chanteloube alluaudite may be heated for a day in air to 990°C without change in its x-ray powder diffraction pattern; it melts close to 1000°C. Ferrodickinsonite, when heated for a day in a platinum envelope in a sealed and evacuated capsule made of Vycor glass tubing, inverts to ferrofillowite at 850°C. The Branchville fillowite when similarly heated to 960° does not show an appreciable change in x-ray powder diffraction pattern. Some cristobalite, presumably developed by the action of Na on the Vycor glass, forms at temperatures of 800°C and above. Unless this involves a significant change in composition, it seems probable that heating to 850°C causes dickinsonite to undergo a reconstructive transformation yielding fillowite.

Heated in air for 46 hr at 885°C, fillowite yields a diffraction pattern rather similar to but far from identical with that of alluaudite. This material is being further investigated. Some of the diffraction patterns from the ferrodickinsonite heated in air above 550°C are not quite identical with that from alluaudite; the small differences will be discussed in a detailed paper to be published elsewhere. The heating experiments were conducted originally to find out at what temperature water assumed to be present was lost; the results indicate that water does not play an essential role in any of these structures (*3*).

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3. The ferrodickinsonite came from the Headden collection of Harvard through the courtesy of Clifford Frondel, who also supplied the fillowite. The Chanteloube alluaudite was furnished by F. A. Bannister of the British Museum, Kensington.

22 December 1954.

Table 1. Silicate-phosphate crystal data. a, b, and c, lengths of the a, b, and c edges of the unit cell; d, spacing.

Cations	Mineral	Crystal system	a (A)	$d_{(100)} = a \sin\beta$ (A)	b (A)	c (A)	$d_{(001)} = c \sin\beta$ (A)	β	Unit cell vol. (A^3)	Specific gravity
Mg Mg	Chondrodite	Monoclinic	10.29	9.73	4.74	7.89	$7.45 = 5 \times 1.49$	109° 02′	364	3.20
Fe^{II} Fe^{II}	Fayalite	Orthorhombic	10.61	10.61	4.81	6.17	$6.17 = 4 \times 1.54$	90° 00′	315	4.14
Li Fe^{II}	Triphylite	Orthorhombic	10.36	10.36	4.68	6.01	$6.01 = 4 \times 1.50$	90° 00′	291	3.58
Na Mn	Natrophilite	Orthorhombic	10.54	10.54	4.98	6.33	$6.33 = 4 \times 1.58$	90° 00′	332	3.47
Na Fe^{III}	Alluaudite	Monoclinic	11.99	10.93	2(6.22)	6.38	$5.81 = 4 \times 1.45$	114° 20′	867	3.58
NaMn	Dickinsonite	Monoclinic	2(12.44)	2(11.98)	2(5.05)	16.68	$15.98 = 10 \times 1.60$	105° 41′	4041	3.41
NaMn	Fillowite*	Rhombohedral	15.25			43.32			8730	3.43

* The optic axial angle (2V) is 30° +. The lattice symmetry lacks a mirror (m), as kindly pointed out by J. D. H. Donnay, but has a 3-fold axis.

"Unpublished" or "In Press"?

Curt Stern [*Science* **119**, 221 (1954)] makes a comment on unpublished articles that has, in my view, wider implications. I submit that it is both conceited and illogical to say, for example, "Further details of these experiments will be published elsewhere" if, in fact, no further paper on the subject has yet been accepted for publication. Whether the paper has been submitted and not accepted, or whether it has not yet been prepared, does not affect the issue; in either event the author is not in a position to state that his further results will be published, unless he arrogates to himself the function and abilities of a prophet! On the other hand, if he wishes to refer to work that Stern calls "actually in press, that is, accepted by a journal," it is both discourteous and inconveniently reticent to refrain from naming the journal. Reference to work at this stage should be given in some such words as "Further details of this investigation are being published elsewhere (*7*)." Then, at the end under "References": A. U. Thor and A. N. Other, *Science*, in press."

A. L. Bacharach

26, Willow Road, London, N.W. 3

17 June 1954.

Our present policy, to which we doubtless have been holding all the more carefully as a result of Bacharach's letter to us, is to ask authors to delete "in press" references when the name of the periodical which has accepted the paper cannot for one reason or another be furnished. As for references of the "in preparation" and "unpublished" sort, these have often been left undisturbed. A reference to unpublished work may be useful for it enables—indeed, should be regarded as inviting—interested readers to communicate directly with the author for details of the work.

Method for Extraction of Substances from Liquid Samples

A modification of the Soxhlet apparatus, which is normally used for extraction of substances from solid samples, makes it possible to use this apparatus to extract substances from liquid material also. Extraction may be performed with solvents either heavier or lighter than the sample. Volatile substances can also be extracted.

When the solvent is lighter than the liquid sample, the modification consists of replacing the extraction thimble with a new device. This device consists of a funnel within a glass tube (Fig. 1). The bottom end of the funnel, though closed, must be perforated with small holes. The size of the funnel and tube will vary according to the size of the Soxhlet apparatus, and therefore depends on the volume of the sample.

If a Soxhlet apparatus with a 250-ml flask is used, the tube may be 8 cm long and 2.5 cm in diameter; the funnel will be 12.5 cm long and 3.5 cm in diameter at its largest point, and 0.8 cm in diameter at the stem. In this case about 20 ml of sample may be used. The sample (for example, aqueous solution) is placed in the tube. The solvent, after being distilled, passes through the funnel and escapes through the holes at the bottom; it then moves upward, passing through the sample, and extracts the material in it and forms a layer on the sample. After having reached the edges of the tube it goes into the Soxhlet apparatus and may be siphoned over to the flask.

When the solvent is heavier than the liquid sample, the modification consists of replacing the extraction thimble with a glass tube having a small side ramification (Fig. 2). As in the case of solvents that are lighter than the liquid sample, the size of the Soxhlet apparatus must vary according to the volume of the sample.

For a 250-ml flask, the tube, which must be slightly larger in diameter at the top, must be 11.5 cm long and 2.5 cm in diameter; its diameter at the widest point must be 3 cm. The side ramification may be 5 cm long and 0.5 cm in diameter. In this instance, about 20 ml of the sample may be used. Before placing the sample in the tube, it is necessary to place some solvent in it in order to prevent drops of the sample from passing through the side ramification. The distilled solvent reaches the surface of the sample; being

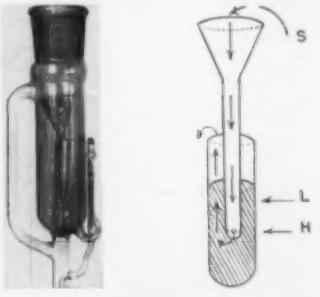

Fig. 1. Modification for solvent *S* lighter than water; *L*, liquid sample; *H*, holes.

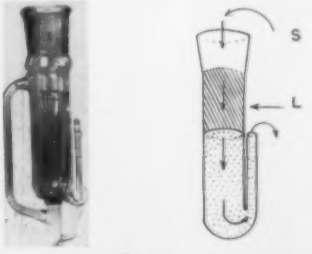

Fig. 2. Modification for solvent *S* heavier than water; *L*, liquid sample.

heavier, it passes down through the sample and extracts the substance. The solvent reaches the side ramification level, goes into the apparatus, and may then be siphoned over to the flask.

This method can be used for several purposes: for extraction of substances from liquid samples; for volatile substances or solvents; for solvents lighter than the samples; for solvents heavier than the samples; for partial extractions (the flask with solvents must be changed during the extraction). The method is being used for extraction of organic acids in feces, in research assisted by financial grants from the São Paulo Jockey Club.

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4 October 1954.

Reingestion in the Hare *Lepus europaeus* Pal.

The domestic rabbit produces special small soft fecal pellets that it takes directly from the anus and swallows whole (*1-4*). In this way, a large part of the food passes twice through the alimentary tract. This behavior, which has been termed *refection* (*2*) or, more appropriately *reingestion* (*5*), also occurs in the wild species (*Oryctolagus cuniculus*) (*6*) and is an important daytime activity of the animal (*5*). The question arises as to how far it is a feature of the biology of lagomorphs generally. A casual observation on a pet hare recorded as long ago as 1895 (*7*) suggested that reingestion also occurred in this species. It was therefore decided to collect wild hares (*Lepus europaeus*) at known times of the day and to examine the contents of the stomach and rectum for evidence of this behavior. Altogether 66 hares were examined from various localities in New Zealand.

Table 1. Incidence of reingestion in *Lepus europaeus*.

Time of day	No. examined	No. with soft feces
2 A.M.	7	0
4 A.M.	2	0
6 A.M.	2	0
8 A.M.	5	2
10 A.M.	5	5
12 Noon	1	1
2 P.M.	0	
4 P.M.	7	2
6 P.M.	8	0
8 P.M.	3	0
10 P.M.	23	0
Midnight	3	0
Total	66	10

The recta of several of these animals contained soft amorphous feces, and apparently identical material was present in the cardiac ends of their stomachs. The fecal origin of this material in the stomach was confirmed by the presence of comparable numbers of oöcysts of intestinal coccidia in both the feces and stomach contents of several of the animals. Table 1 groups all the animals examined into 2-hr periods throughout the day according to the time of death. It can be seen that the amorphous feces were found only in hares killed between 6 A.M. and 4 P.M. and that they were found in the recta of all animals killed between 8 A.M. and 12 noon. This corresponds almost exactly to the time when reingestion takes place in the rabbit (*5*) and shows that the hare has a similar well-defined diel rhythm.

It now seems likely that reingestion will be found as a normal feature of lagomorph biology. So far only two European species *O. cuniculus* and *L. europaeus* have been studied, and no information has been recorded on this aspect of the biology of any of the many American species. Reingestion could easily be overlooked, particularly if the soft feces are semi-liquid, as in the European hare, since then they might be taken as an indication of some intestinal disorder; however, the regularity in the time of their appearance should disclose their real nature.

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18 January 1955.

Science does not know its debt to imagination.—R. W. EMERSON.

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Forest Service Observes Its Golden Anniversary

FIFTY years ago 1 February, the Forest Service was established in the Department of Agriculture. It inherited the functions of the old Bureau of Forestry and the administration of the Forest Reserves, now called national forests. Gifford Pinchot was named its chief.

Pinchot was not the first forester in this country, nor was the Forest Service the first federal agency to deal with forests, but he and his Forest Service were the first to propound "conservation of forest resources through wise use."

It has taken 50 years and the combined efforts of the state and federal forest services, forestry schools, and conservation-minded industries, individuals, and organizations to put across the theory that trees can be managed as a crop.

Today the Forest Service still carries on the work it inherited. It aids private landowners through cooperative programs with the state which provide for fire protection, distribution of planting stock at nominal cost, and technical-on-the-ground assistance in forest management. It also provides leadership in the control of forest insects and diseases. It administers 181 million acres of national forests. It carries on research.

One of the great achievements in forestry during the past 50 years is the progress made in putting Pinchot's sustained-yield idea into effect on privately owned lands. Today many large timber companies hire trained foresters to manage their woodlands, and some grow their own seedlings to replant burned-over and barren land. An increasing number of small landowners are getting professional advice on how to manage their forests and are planting more trees each year.

Some other accomplishments in forest conservation are (i) organized fire protection on 374 million acres, or all but 50 million acres, of forests in this country; (ii) development of new pulping processes particularly adaptable to hardwoods which have little other commercial value; (iii) production of kraft paper and newsprint from southern pine, which also has limited commercial use; (iv) construction of structural sandwich wood, which resulted in the mosquito bomber of World War II and is now used in freight cars; (v) invention of the fire-danger meter, which indicates the flammability of the forest through integration of data on humidity, wind velocity, moisture in the forest, days since the last rain, and the season of the year; with this information foresters can estimate how fast a fire is likely to spread and how hard it will be to control, plan which towers to man and how many firefighters to alert; (vi) use of air-borne parachutists to fight fires in the remote areas of the West; by getting to fires while they are small, these smokejumpers have saved thousands of dollors' worth of timber; (vii) improvement in the turpentining of longleaf and slash pine trees through application of sulfuric acid, which takes less labor, wastes less wood, and results in a higher yield of gum; (viii) rehabilitation of the Wasatch Range in Davis County, Utah, through the construction of contour trenches and revegetation on the high country; the land that had been so overgrazed that mudflows and floods almost ruined the valley below in the 1920's is now covered with grass and herbaceous growth that holds the water and has almost eliminated floods; (ix) improvement of national forest rangeland with 29,250 mi of fence, 3260 mi of driveways, 17,700 water developments, reseeding of 550,600 acres, removal of sagebrush from 8568 acres and juniper from 12,000 acres, and better management of livestock on the range.

Thus progress has been made in the conservation of forest resources. Forest conservationists in the 50 years ahead, however, will be faced with the problem of producing more wood, water, and forage from forest lands to meet the increased demands of a growing population. This necessitates better utilization of present supplies, plus planting millions of acres of rundown and burned-over potential forest land, management of millions of acres of forest, especially those in small ownerships, and adopting a system of quick detection and control of insects and diseases to prevent timber losses.

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Meetings & Conferences

March

25–26. Michigan Acad. of Science, Arts and Letters, E. Lansing, Mich. (George M. McEwen, Univ. of Mich., Ann Arbor.)

28–31. American Assoc. of Petroleum Geologists, New York, N.Y. (E. H. Powers, Box 670, Fort Worth, Tex.)

28–1. Western Metal Exposition, 9th, Los Angeles, Calif. (W. H. Eisenman, 7301 Euclid Ave., Cleveland 3, Ohio.)

29–7. American Chemical Soc., 127th national, Cincinnati, Ohio. (A. H. Emery, 1155 16 St., NW, Washington 6.)

31–2. Soc. of Research in Child Development Monticello, Ill. (C. B. Stendler, College of Education, Univ. of Illinois, Urbana.)

April

1–5. Japan Medical Cong., Kyoto. (M. Goto, Univ. Hospital, Kyoto Univ., Kyoto.)

2. Kappa Delta Pi, Cleveland, Ohio. (E. I. F. Williams, 238 E. Perry St., Tiffin, Ohio.)

3–6. American Astronomical Soc., Princeton, N.J. (C. M. Huffer, Washburn Observatory, Madison 6, Wis.)

3–7. American College Personnel Assoc., Chicago, Ill. (C. Evans, Univ. of Indiana, Bloomington.)

4. American Educational Research Assoc., Cleveland, Ohio. (F. W. Hubbard, 1201 16 St., NW, Washington, 6, D.C.)

4–5. Atomic Industry Conf., San Francisco, Calif. (Atomic Industrial Forum, 260 Madison Ave., New York, N.Y.; or Stanford Research Inst., Stanford, Calif.)

4–5. Histochemical Soc. Symposium, Philadelphia, Pa. (A. B. Novikoff, Waldemar Medical Research Foundation, Port Washington, N.Y.)

4–5. National Gastrointestinal Cancer Conf., sixth, New York, N.Y. (Morris K. Barrett, National Institutes of Health, Bethesda 14, Md.)

4–6. American Assoc. of Physical Anthropologists, Philadelphia, Pa. (J. L. Angel, Jefferson Medical College, 307 S. 11 St., Philadelphia 7.)

4–6. International Meeting on Phlebology, 3rd, Aix-en-Provence, France. (F. Beurier, 4 Cours Mirabeau, Aix-en-Provence.)

5. Microcirculatory Conf. for Physiology and Pathology, 2nd, sponsored by American Association of Anatomists, Philadelphia, Pa. (G. P. Fulton, College of Liberal Arts, Boston Univ., 725 Commonwealth Ave., Boston 15.)

5. Tissue Culture Assoc., annual, Philadelphia, Pa. (M. R. Murray, TCA, College of Physicians and Surgeons, New York 32, N.Y.)

5–7. French Medical Cong. on Thrombosis and New Antibiotics, 30th, Algiers, Algeria. (R. Raynaud, 12 Rue A.-Leluch, Algiers.)

5–7. Radio Technical Commission for Aeronautics, spring assembly meeting, Los Angeles, Calif. (RTCA, Room 2036, Building T–5, 16th and Constitution Ave., NW, Washington 25, D.C.)

6–8. American Assoc. of Anatomists, Philadelphia, Pa. (N. L. Hoerr, 2109 Adelbert Rd., Cleveland 6, Ohio.)

8–9. Pennsylvania Academy of Science, Philadelphia, Pa. (K. Dearolf, Public Museum and Art Gallery, Reading.)

9. South Carolina Acad. of Science, Columbia, S.C. (H. W. Freeman, Univ. of South Carolina, Columbia.)

10–15. American Inst. of Homeopathy, Washington, D.C. (W. R. Huntsman, AIH, 1601 Chestnut St., Philadelphia 3, Pa.)

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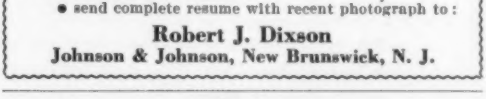

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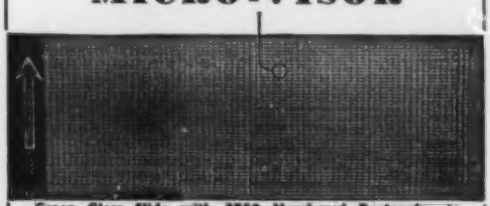

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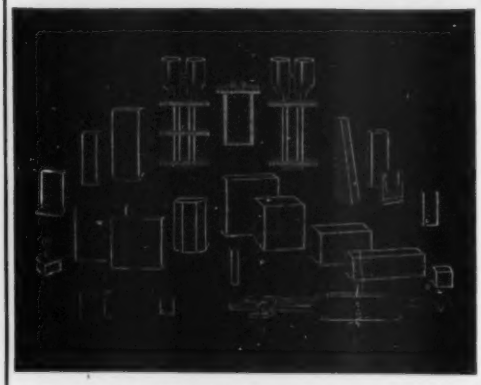

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